Vol. 34, No. 6 June, 1937

THE
PSYCHOLOGICAL BULLETIN

EXPERIMENTAL STUDIES OF THOUGHT AND REASONING

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Introduction

A review of the accumulation of 10 years' experimental work on thinking presents unique problems, not only because the field itself is hard to define, but because it is hard to draw a line between studies of an experimental nature and ones which are purely speculative.* Yet no field seems to be in greater need of summary and evaluation.

I. The Nature of the Thinking Process

Thinking as Directed Activity. Since the time of the classical experiments of the Würzburg School virtually all investigators have recognized, implicitly if not explicitly, that a major feature of all thinking processes is their directed character. Pratt (79) in his review of the literature noted the unanimity of opinion upon this point and since then no dissenting voice has been raised.

A number of studies have dealt specifically with the effects of

* The period covered dates from Pratt's review in 1928 (79). The present review includes primarily studies of problem-solving, concept formation, and generalization. It excludes, however, studies of children's conceptual systems, problem-solving in animals, and group thinking since these constitute fairly extensive separate fields in themselves. In endeavoring to make a usable classification of the material, a separation has occasionally been forced between studies which may seem to the reader to belong under a single heading. The material can be readily regrouped to suit the reader's need or his systematic position. For studies published in Chinese, Japanese, Polish, or Russian, the *Psychological Abstracts* have been relied upon rather than the original sources.

sets or attitudes upon thinking. Wilhelm (**105**) has investigated the effects of instructions upon syllogistic reasoning and found that subjects draw inferences from premises only when they have a set to do so. This set may occasionally arise independently of the instructions; a set to draw inferences may also carry over from one situation to another in which no such instructions are given. Siipola (**92**) has also found that under certain conditions a set may carry over spontaneously to a subsequent task. When subjects were told, for example, to look for the names of animals and birds in equivocal items flashed upon a screen, the set aroused by these instructions persisted and strongly influenced the subjects' performance in the task of filling in skeleton words, even though the instructions for the latter task were merely to complete the skeletons as rapidly as possible.

Tendencies toward certain kinds or methods of solution may be aroused by the nature of the material itself. Rees and Israel (**81**) have shown that sets established by peculiarities of the material (lists of anagrams) may be equally as effective in determining the nature of solutions as sets aroused by specific instructions. Furthermore, certain types of sets may be completely unconscious yet operate with full effectiveness. Sells (**88**) has demonstrated the effectiveness of unconscious sets in syllogistic reasoning. His concept of "atmosphere effect" is defined as "a temporary set of the individual, arising within a situation, to complete a task with that one of several alternative responses which is most similar to the general trend or tone of the whole situation" (**88**, p. 7). In none of the introspective records of his subjects did Sells find anything which might be characterized as an "atmosphere experience," yet his data show that the existence of such a set is indubitable.

Claparède (**16, 17**), Duncker (**26**), and Maier (**59, 60**) have all employed a conception of direction which stresses the notion that a direction or "need" is produced by the intrinsic properties of problems of certain types. The kind of solution which must be found is demanded by the nature of the problem itself rather than determined by instructions. The specific contributions of these investigators are treated in other sections of this article.

Other investigators who have emphasized the rôle of direction or sets and whose studies are treated elsewhere in this review are Bulbrook (**12**), Ewert and Lambert (**28**), and Sullivan (**97**). Morgan (**68**) has also stressed "the distinctive influence of the goal." Siebert (**91**) found that wishes, attitudes, and expectations influenced the subject's experience of imaginary situations.

Harrower (33) has shown that incomplete jokes, like more specifically problematic situations, have properties which demand completion or "closure." The instruction (which is often unnecessary) to finish a joke "gives no indication of *how* it is to be finished. All answers which come to mind must come as the result of specific properties of the joke as such" (33, p. 69). In an extensive investigation of which we report only a part, Harrower has found that some jokes were completed by practically all subjects in exactly the same fashion whereas other jokes were completed equally well by either of 2 contradictory ideas. Using the latter type of joke, it was found that the direction of completion could be controlled by the effects of jokes previously read to the subject. In another experiment, the answers to incomplete jokes were either "embedded" in connected prose or were members of a series of unrelated sentences. It was found that the answers which were embedded in prose were almost never used to complete jokes but the frequency with which answers from the loose sentences were utilized varied with the specificity of the instructions given.

The influence of directions both within the problem and extrinsic to it will be obvious in the results of many studies reported below.

The Occurrence of the Problem Solution. Experimental investigations of the occurrence of the problem solution have centered around the insight *versus* trial and error controversy to an extent which is perhaps disproportionate to its significance. The general method of a large number of studies is simply to place subjects in problem situations and from an observation of their behavior and a record of their remarks to conclude that insight is or is not the determining factor in the solution of the problem. Since the conclusion seems often to depend on the theoretical predilection of the investigator, and since there are few if any criteria of insight which are agreed upon, such studies add very little to our understanding of the processes involved.

A number of studies, of which Alpert's (4) is the earliest, have employed material of the type used by Köhler (55) in his studies of the problem-solving of apes. Alpert presented children (19 to 49 months of age) with 9 situations in which a lure was placed in such a fashion that the child had to employ some instrument (sticks, boxes, chairs) in order to obtain it. Alpert distinguished the following types of solution: (1) solution with immediate insight; (2) solution with gradual insight which may be partial or complete; and (3) solution with sudden insight which may mature during exposure

or between exposures. Brainerd (**10**) studied the reactions of a child (aged 31 months) to problems adapted from Köhler's. While he did not commit himself to an explanation in terms of insight, he concluded that the child's method of attack and solution was very similar to that of the apes. Problems of the same general type have been presented to idiots by Aldrich and Doll (**2, 3**). They reported that the solutions were often sudden and complete, thereby satisfying a criterion of insight as used by Köhler and Alpert. They hesitate, however, to attribute insight to idiots. Jones (**54**), in a general report of research carried out at the University of California, reported that behavior in situations of the same type as Alpert's tended to be of the "unreflective trial and error type" rather than exhibiting insight. Basing her conclusions on a study similar to Alpert's, Matheson (**64**) perhaps best represents the view which embraces neither trial and error nor insight as an explanatory concept. ". . . Understanding of a situation exists in *degrees* and . . . the complete understanding which Köhler characterizes as insight represents one extreme of the distribution when all cases are considered" (**64**, p. 260).

Other studies of insight in children have employed a variety of experimental materials. Harter (**34**) presented non-verbal performance tests to children from 2 to 5 years of age and found that with this type of material overt trial and error was the most common form of attack. This held true for adults as well as for children. Kreezer and Dallenbach (**56**) required children (5 to 7½ years) to give the "opposites" of good, big, white, etc. All the children who learned the meaning of the relation of opposition learned it "immediately upon gaining insight into the problem." As used by Kreezer and Dallenbach insight is not an explanatory concept but rather a description of the sudden shift from 100% failure to 0%.

Many of the experimental studies of problem solution in adults are similar to those which we have listed above in that they are designed to test no hypothesis and in that the method is simply that of presenting the subjects with problem situations. In an experiment performed by Patton (**76**) the subject had to learn to make a relative judgment concerning pairs of light stimuli of variable intensity. Solution depended upon the subject's discovering a principle. Patton concluded that his results could only be interpreted by reference to some concept of insight. He noted that the emergence of insight may be gradual as well as abrupt. Drever (**25**) similarly reported that the discovery of a principle is gradual. At the opposite pole of

opinion, Bulbrook (12) concluded that her experiments with problems generally alleged to be insightful in type showed no evidence for "a characteristic process, operation, form of conditioning or mode of discovery which we could with propriety distinguish as 'insight'" (12, p. 453). Her interpretations stressed the importance of hypotheses and she reported that the functions most commonly found in her solutions were search, apprehension, inspection and comment, and comprehension. Yet, as she herself pointed out, many of the instances presented in her protocols could be used by other investigators as evidence of insight in one of its many senses.

Hartmann (35), in a study of trial and error and insight, employed problems which could be solved only by the discovery of a general principle. His conclusion was that "trial and error activity when applied to problems which are capable of yielding a general principle eventuates in insight. . . . The required act is firmly learned as soon as, and only when, the insight has been achieved" (35, p. 677). Hartmann's results confirmed Alpert's classification of the varieties of insight. For other studies of insight, see Kubo (57) who has stressed the importance of "foresight" as well as "insight," and Henry (41) who found little evidence of insight in students' solutions of originals in geometry.

A criterion of insight which has been frequently employed is that of transfer. Of the studies mentioned above, Alpert (4), Matheson (64), and Patton (76) reported that transfer from one problem to a similar one occurred. Transfer also occurs, of course, with problem material which is not capable of insightful solution. For studies of transfer from one Peterson rational learning problem to another, see Bunch (13) and McGeoch and Oberschelp (67).

Roberts (83, 84) presented children with a series of multiple choice problems all involving the same principle; in every situation the door of the correct box matched (in color, form, etc.) the object which constituted the lure. A large proportion of the children made immediate application of the principle once they had learned it in the first series of problems. Roberts did not, however, use these results as evidence of insight. In her view, the concept "adds nothing to the understanding of the process of solution" (84, p. 70).

The experiments of Maier (59, 60) and Duncker (26) differ from many of the other studies of reasoning in that they derive from a theoretical setting and in that they are usually designed to test an hypothesis. Maier (61) contends that "the term reasoning implies that something new has been brought about and that, in some way,

past experiences have been manipulated" (61, p. 336). He postulates 2 distinct abilities, reasoning and learning. According to Maier, learning involves the association of contiguous experiences; repetition is necessary for the relationship to be established. Reasoning, on the other hand, is a reorganization of isolated experiences in terms of a goal. These integrations "arise without previous experience and consequently are new. They are not the product of trial and error" (61, p. 336). Insight, in Maier's theory, is "the experience an organism has when two or more isolated experiences come together" (61, p. 337). Maier has demonstrated by experiments (59) that the integration of experiences must take place in terms of a goal. Mere conscious presence of the experiences necessary to solve a problem is not sufficient; the subject must also have the correct "direction." To test his hypothesis, Maier broke up the solution of a problem (of a type requiring manipulation of concrete materials) into 3 parts and gave these to his subjects as separate experiences. Only 1 out of 52 subjects solved the problem. To another group, he gave not only the experiences but also the correct direction or way of looking at the problem; of this group, 8 out of 22 solved the problem immediately. Thus the subject is unable to solve a problem unless he approaches it with the correct direction; without the direction, he cannot reorganize the necessary experiences. On the other hand, for this particular problem, direction alone is not enough. No member of a group which was given only the direction succeeded in solving the problem.

In a later study (60), Maier found that the appearance of the solution in consciousness was nearly always sudden and complete and the subject was unable to report what cues brought it about, even though the cues may actually have been "helps" given by the experimenter.

Duncker (26) is in essential agreement with Maier with reference to the importance of the subject's seeing in the problem the correct difficulty to be overcome. He feels, however, that Maier does not need the term direction as distinct from problem, since the way the subject sees the problem is simply the first phase of solution. The most common type of solution of a practical ("*praktisch-technisch*") problem is that of first seeing the nature of the difficulty to be overcome and then casting about for some means to overcome it. A less common type is that of surveying the available material and from it deriving the appropriate method of solution. This latter approach

Duncker found to be common in solving mathematical problems. Problem solution of both types depends upon the presence of insight.

Claparède (**16, 17**) offers no theory of the occurrence of the problem solution or "act of intelligence," but rather a functional description of the formation of an hypothesis. The problem materials which he employed included skeleton words to be completed, comic pictures for which the subject had to find a title, riddles, and hidden pictures. The problem situation as perceived by the subject arouses a "question" or "need." An hypothesis may occur by simple comprehension of the demands of the situation, as in such relatively simple problems as skeleton words. If the hypothesis does not occur immediately, ideational trial and error, which may be more or less blind or may be systematic, occurs. Chance may play an important part in the solution of the problem but the solving process is steered throughout by the "question" or "need." The "need" exercises selective action upon the elements present and makes use of those which harmonize with it. This process Claparède calls "implication" and distinguishes from association in that it is controlled by the "need." He concludes, however, that neither he nor any other investigator has really explained the mechanism whereby the "need" produces the new hypothesis. The question of *how* the solution to a problem is brought about remains as yet unsolved.

Causes of Errors in Thinking. Any complete theory of productive thinking must include an explanation of errors arising within the process. Maier (**62**) has suggested on the basis of his theory that many errors may be due to the inflexibility of the reasoner's set—the tendency of an habitual direction to block the correct direction. He has tested this hypothesis by comparing the solution of problems by 2 groups of subjects, 1 group having received previously a lecture containing warnings against inflexibility. The warned group was superior to the control group. Other investigators have noticed that a rigid set frequently seems to block other sets (Rees and Israel, **81**) and even to prevent the subject from seeing details which ought to make him reject it (Claparède, **16, 17**).

Duncker (**26**) offers a theory of errors which is quite similar to that of Maier. He suggests that familiar functions of the material employed will suggest themselves and prevent a "recentering" of the object to a new function, thereby throwing the subject off the track of the solution. To test this hypothesis, he has performed a number of experiments of the following type. The subject is required to attach 3 candles to a door. On a nearby table are several thumb-

tacks, the candles, and the critical objects, 3 little cardboard boxes. In one form of the experiment, the boxes are empty. But in another, the boxes are filled with matches, thumb-tacks, and the candles. The subject must see the boxes as candle-holders to be tacked to the door. When the boxes were presented as containers, only 43% of the subjects succeeded in solving the problem. When the boxes were presented empty, however, 100% of the subjects solved it. Duncker has worked out a number of rules relating ease of using an object in a new function to the nature of its previous function. He speaks of the object as "functionally bound" when its old properties prevent a new function from occurring. Devnich (**23**) arrived at a similar conclusion with anagrams. When the anagram was presented in the form of a meaningful word, the chances of solution were fewer than when it was presented in meaningless form. Hollingworth (**44**) has performed this experiment with less conclusive results.

A related hypothesis is that errors are caused by an "interpretative" approach in which old meanings and associations determine the response, rather than an analysis of the data at hand. This idea was suggested by Chant (**14**), who found that subjects could be divided into 2 groups: (1) those who devote most of their time to comparing the data at hand; (2) those who make inferences from individual cases on the basis of previous associations. Errors could generally be attributed to the latter procedure.

An elaborate study of errors in syllogistic reasoning has been made by Sells (**88**) and by Woodworth and Sells (**108**), who have the distinction of having formulated an hypothesis, made specific predictions of results to be expected on the basis of it, and tested the predictions in a critical experiment. They suggest that the premises have an "atmosphere" which is strong or weak (all-yes, all-no, or some-yes, some-no) and which calls for the same type of conclusion. They predict exactly what answers are to be expected for each member of a collection of syllogisms. The conclusions favored by atmosphere were generally accepted, whether valid or invalid. Several secondary hypotheses are suggested and checked.

Müller and Pilzecker's "law of substitution" as an explanation of errors in "task-directed" problems has been defended by Wilcocks (**104**) against Selz (**89**). Cases of errors in his protocols which led him to accept the law of substitution as a supplement to Selz's notion of partial efficiency of the anticipatory schema showed that "one solution is anticipated but that another offers itself on the basis of existing dispositional knowledge." Elements only of the

anticipated conclusion came to mind; dispositional knowledge for another conclusion containing these elements had stronger "reproductive preparedness."

Symonds (**98**) analyzes various types of thought processes, such as explanation, generalizing, and problem-solving into steps or stages, and explains errors and failures on the basis of this analysis. For instance, he says that the first step in solving a problem is that of isolating and defining the values operating in the situation; and a possible explanation for failure may be failure to isolate and define the values sought. Other relevant experiments are those by Wilhelm (**106**), Eidens (**27**), Wilkins (**107**), and Budkiewicz (**11**).

Creative Thinking. Although there seems little reason to doubt that the creative thinking of artists and inventors is essentially similar to ordinary productive thinking and problem-solving, there has always been a special interest in their achievements, perhaps because the aspect of novelty is particularly apparent in them. Hutchinson (**45**) reports that there is no psychological literature of creative thinking and suggests that the psychologist turn to the material from "philosophy, intuition, mysticism, literary criticism, art, invention, and genius." For the point of view that creative activity is not essentially different from other activity, see Netschajeff (**72**).

An experimental approach to the problem has been attempted by Patrick (**73**). She secured a group of 55 poets and a control group of non-poets and recorded their remarks and observations as they each wrote a poem in her presence. She also obtained questionnaire results on the work habits of the poets. Examination of the results of both groups led her to conclude that Wallas' (**100**) 4 stages (preparation, incubation, illumination, and verification) were characteristic of creative thought.

Two very interesting questionnaire studies, neither of them done by psychologists, offer some corroboration of Patrick's conclusion.

Platt and Baker (**78**) secured information from a number of scientists on the relation of the "hunch" to research. The scientists stressed the necessity of preliminary intensive study of the problem, followed by a period of rest corresponding to Wallas' incubation. The solution usually came suddenly when they were not formally working on the problem. Their insistence on the necessity for temporary abandonment to allow new sets to develop and on the importance of discussion in avoiding wrong assumptions coincides very well with the results of experimental studies of errors in thinking. Rossman (**86**) made a questionnaire study of the methods of work of inventors. The inventors emphasized the importance of a need, elaborate preparation including collection of information and critical surveys of available solutions, a period of incubation,

and (following the occurrence of the new idea) testing of the solution. Rossman is especially impressed by the rôle of the need, and this again corresponds with experimental results. These studies seem particularly fruitful as a check on conclusions based on laboratory problem-solving.

Studies of Concept Formation and Abstraction. Smoke (**94, 95, 96**) has criticized some of the older work on concept formation for employing as experimental material common elements hidden in a group of figures. He defines a concept as a response to relationships common to two or more stimulus patterns, and he has performed experiments in which relational concepts were formed. A series of geometrical designs were exposed to the subject, who looked for a common pattern in them. Smoke found that giving the subject instances contrasting with the concept as well as positive ones did not materially aid learning, either with serial or simultaneous presentation of the material. He also found that subjects often were able to use the concepts correctly in a test series, although they could not give an adequate verbal formulation of them. This result was verified by Heidbreder (**39, 40**) and Radlinska (**80**). Smoke and Heidbreder further agree in finding instances of both sudden and gradual concept formation. Roberts (**84**) and Hazlitt (**36**) found that children are able to apply principles correctly when they cannot state them.

An experimental technique for studying development of concepts, originated by Ach (**1**), has been perfected by later investigators, especially Sakharov (**87**) and Hanfmann and Kasanin (**32**). As used by Hanfmann and Kasanin, the technique consists of confronting the subject with a number of objects (geometrical solids) varying in shape, color, size, etc. The subject is told that there are 4 different kinds of objects, that each kind has a name, and that he is to separate these 4 kinds. The subject must discover that each class or name refers to definite combinations of characteristics of the objects, such as *tall and flat,* for which there is no adequate single term. In analyzing this task, Hanfmann and Kasanin have pointed out 3 major aspects of conceptual thinking: the importance of the attitude of looking for categories, the recognition of many possibilities rather than merely the first one to occur, and the consideration of the total system. Weigl (**101, 102**), using a somewhat similar technique, also called attention to the necessity of a categorical attitude, pointing out that abstraction does not occur through mere grouping or arranging of objects.

Difficulty in shifting from one manner of conceiving an object to

another has been studied in normal subjects by Gengerelli (**30**). The subjects learned to speak a letter for a common element present in a group of forms, and later learned new concepts for the same forms by learning to respond to a different element in each one. He found that it is more difficult to conceive an object a second way after having learned to conceive it in a first way.

The effect of instructions on generalization has been studied by several investigators. Sullivan (**97**) had subjects of different age ranges learn nonsense names for geometric figures as common elements, giving 3 types of instruction: (1) merely to learn the names of the pictures; (2) to notice common elements and look for a generalization; (3) with demonstration of the generalization by the experimenter. She found that the chance of generalizing was greater and the speed of generalizing greater, when it occurred with the third type, although retention was superior with the first type. Ewert and Lambert (**28**) found that a rule for solution of a disc problem of various degrees of difficulty could be better formulated the more specific the instructions received by the subjects in advance, although a demonstration given by the experimenter in addition to full instructions did not help. Fowler (**29**), in investigating whether concepts were best formed by beginning with individual cases or not, found that beginning with the generalization and proceeding to individual cases was most effective, when the generalization was combined with explanation. This conclusion in a sense confirms the other 2 studies, since in their most effective conditions the instructions included presentation of the required principle.

II. The Relation of Thinking to Other Processes

The Rôle of Imagery in Thinking. With the earliest attempts of the Würzburg school to bring thinking into the laboratory, the problem of the "content" of thought received attention. In Pratt's summary 10 years ago, one of the major conclusions drawn from the assembled studies concerned the "impalpable" nature of this content. It is interesting to note that in recent years this problem has excited little work. Two studies, those of Bowers (**9**) and Peillaube (**77**), have confirmed the earlier work. Bowers found that problems which were more easily visualized in imagination were no more easily solved. Also, persons possessed of vivid visual imagery had no advantage in problems apparently designed to make necessary the use of visual imagery. Peillaube, in the tradition of the Würzburg

school, found that there was psychic content distinct from images or any content of a sensorial nature.

Motor Accompaniments of Thought. Studies of the motor concomitants of thinking have agreed in finding a positive relationship between mental and motor processes. The motor processes have been measured by such varied indicators as action potentials, galvanic skin reflex, winking rate, grip tension, and gross bodily movement. Reiter (**82**) measured gross bodily movement, but no results are reported. Jacobson (**46, 47, 48, 49, 50, 51, 52, 53**) measured action potentials in specific muscle groups when the subject was engaging in a particular mental activity such as imagination or problem-solving.

He employed subjects previously trained to relax. He found that when a subject was engaged in mental multiplication, or thinking of such general concepts as "everlasting," or recalling the words of a song, muscular contractions in the tongue and lips characteristically appeared. Furthermore, there was some indication that the patterns of contraction were similar when a group of words was imagined and when the words were actually whispered. Neuromuscular activity during thinking or imagining appeared to be specific to the muscle group which would actually be involved in the performance of a particular act. For instance, no action potentials were registered in the tongue and lips when the subject was told to imagine the appearance of the Eiffel tower; but visual imagination of this sort was accompanied by contraction of the ocular musculature. Control tests were given in which the subject was told not to imagine, or instructed to imagine an act which would be performed with different muscle groups from those under investigation.

Jacobson concluded that contraction of specific muscles is not only concomitant with the occurrence of certain mental activities, but is essential to their occurrence, since imagination of a particular act failed to occur if the muscles involved in it were completely relaxed. Jacobson's conclusions receive some confirmation in the work of Max (**66**), who studied action currents in deaf mutes. He found that the onset of dreams could be detected by the appearance of large action currents in the arm and finger muscles of these subjects, while hearing subjects gave no action currents from the fingers during dreams.

Clites has made a series of studies of somatic activities in relation to successful and unsuccessful problem-solving (**18, 19, 20**). He found that successful problem-solving was accompanied by an increase in action potentials as compared with a control period, while unsuccessful problem-solving showed little difference.

Overt movements of muscles of the forearm decrease in successful problem-solving and grip pressure is increased at first and relaxed as the solution occurs.

In unsuccessful problem-solving, the same amount of forearm movement and the same grip tension are maintained as during the initial rest. Measurements of winking rate showed an increase with a successful solving process, but a constant rate in subjects who persevered but failed. The increase seemed to be more consistently present with difficult mental tasks than with easier ones. Measurements of skin resistance to an electric current showed a decrease of resistance during successful problem-solving, probably indicating increased activity of the sweat glands. For failing subjects, there is little indication of increased sympathetic activity during problem-solving.

These recent experiments tend to confirm the possibility of a motor theory of thinking, although experiments in the previous decade on the whole did not. This discrepancy may be due to the inadequacy of older techniques of registration, as Max has suggested (**65**).

Reason and Emotion. The traditional opposition of reason and emotion has received little experimental clarification, although the problem seems to be a peculiarly interesting one. The demonstration of the important rôle of needs and direction in thinking makes this problem all the more tempting, since a superficial inconsistency occurs if one recognizes the necessity of motivation and, at the same time, the inhibiting effect of accompanying emotion. Clites (**18**, **19**, **20**) found evidence of increased activity of the sympathetic nervous system during successful problem-solving, possibly indicating that there was an emotional accompaniment in successful problem-solving which was lacking in unsuccessful problem-solving. The inference is at least sufficiently suggestive to be worth further research.

Patrick (**73**, **74**) attacked the problem directly. He used an adaptation of Hamilton's choice problem and in control experiments found that his subjects generally made a superior type of response involving a rational inference. But when emotional stimuli (cold showers, shocks, etc.) were introduced along with the problem, the most frequent response shifted to the lowest type, which involved repeating attempts already proved unsuccessful. Perhaps in contradiction to Clites, then, he has shown emotional excitement and rational behavior to be incompatible.

Reasoning and Learning. In spite of the systematic importance of this problem, few experimental studies with human subjects have been directed at its solution. Maier (**61**) has discussed the problem and performed experiments which he believes uphold his contention that the 2 are separate abilities (see pp. 331–332). A number of studies contribute correlations between problem-solving and learning of various types. Billings (**8**) found correlations ranging from .35 to

.59 between information and problem-solving in various fields. Arons (5) found no significant correlation between ability to memorize series of playing cards and ability to formulate generalizations in a multiple choice problem. Ewert and Lambert (28) found a positive correlation between maze-learning and generalizing ability which became significant when corresponding full instructions were given in both cases. Bedell (7) gave paragraphs containing scientific information to students in eighth and ninth grades, and tested factual recall and ability to form inferences from the paragraphs. Analysis of the results suggested that ability to recall and ability to infer are different, but not unrelated.

Reasoning and Intelligence. It seems obvious that there must be a very close relation between what we commonly call reasoning and intelligence. Certainly to the layman they are identical, and to the psychologist it is apparent that there is some identity between laboratory reasoning and what is measured on tests of intelligence. Billings (8) has a challenging opinion to offer with respect to the status of problem-solving as an ability. He set out to discover whether a person is as good a reasoner in one academic field as another, and gave his subjects problems from such fields as arithmetic, geometry, physics, mechanics, sociology, etc. Materials necessary for solution were taught the subject first. Correlations between these various fields ran from .53 to .78 and between problem-solving in these fields and Army Alpha the correlations ranged between .42 and .59. Billings finds that the correlations satisfy the criterion of the Tetrad Equation, and concludes that problem-solving is a general factor, if not "intelligence" itself. Sells (88) and Wilkins (107) find significant correlations between syllogistic reasoning and intelligence (.711 and .578), but both find a correlation of the order of .50 between abstract and concrete syllogism scores when intelligence is partialled out. Sells interprets his results as indicating "two separate ability factors, possibly a reasoning factor and an intelligence factor."

On the whole, results have been consistent in showing a significant relationship between intelligence and various types of thinking.

Smoke (94) reports a correlation of .52 between speed of concept formation and intelligence; Ewert and Lambert (28), correlations ranging from .86 to .96 between various criteria of generalizing and intelligence; and Roslow (85), multiple correlations ranging from .32 to .81 between a battery of rational learning problems and various intelligence tests. A multiple factor analysis of Roslow's results revealed the presence of a common factor in the problems and intelligence tests. Bedell (7) found that students in the lower quartile of their group in intelligence scored scarcely better than chance in an inference test.

Personality Differences and Thinking. Everyday observation suggests that there are many ways of attacking a problem and that in problem situations individuals differ from one another in ways which might be predicted from their behavior in other life situations. Only a few studies have been devoted to investigation of such a relationship. Wilhelm (105) compared the thinking of introverts and extraverts. He found the introverts more analytic and systematic, taking all the intermediate steps, while the extraverts showed a tendency to jump to a conclusion and to prefer problems with a very definite content. Dieter (24) studied thought forms as related to the basic structure of the personality, and found 2 fundamental thinking types, the formalistic and the objective, the first concerning itself primarily with form and the second with content. Vigotsky (99) and Hanfmann and Kasanin (32) studied the concept formation of schizophrenic patients. Vigotsky contended that schizophrenics no longer think conceptually, but in "complexes"—groups composed of members which are all individuals rather than bearers of a common characteristic. Hanfmann and Kasanin have checked his results and secured quantitative records. They found that the achievement of schizophrenic patients in the test described above was consistently poorer than that of normal subjects, especially when patients with a college education were compared with normal college graduates.

III. Factors Determining Efficiency of Thinking

Training. In the main, the investigations upon this problem support the conclusion that previous training is effective in improving "logicality of thought" only in so far as the training situations and the test situations are similar either with respect to the material employed or with respect to appropriate methods of attack. White (103) gave special weekly lessons in logic to one of two groups of boys of the same age (13 years) and intelligence. He found that at the end of 3 months the experimental group showed a significant superiority over the control group on a test of formal reasoning. Smaller differences between the groups were found on Ballard's English Construction Test and a composition. Hill (42) found that training in different types of evidential material resulted in specific gains for each type but that the gain was transferable only in those cases where the method of attack facilitated the handling of the new type of material. Shendarkar (90) used several different methods in training subjects to solve arithmetic problems and found that the transfer to several reasoning tests was roughly proportional

to the degree of similarity between training and test materials. He found no transfer to "general reasoning," as tested by Burt's Reasoning Test. Bauer (6) found that training in the task of constructing definitions of words had the effect of increasing the number of adequate solutions and decreasing the number of errors.

Chant (14), in an experiment on reasoning in adults, classified his subjects into 2 groups on the basis of their approach to the problems (see above). The only academic difference between the 2 groups was that a larger proportion of the more successful group had had training in experimental science. Chant concluded that this disparity in training accounted for the differences in method.

Sinclair and Tolman (93) compared the scores of liberal arts students and students in an institute of technology on the Watson Test of Fairmindedness and found that the latter were consistently superior to (less prejudiced than) the former. The conclusion is made, however, that the difference between the 2 schools was to be ascribed not to scientific training but rather to differences in the type of student entering the 2 schools.

Length of Problem. The influence of length of the problem to be solved upon the difficulty of problem solution has received attention. McGeoch and Oberschelp (67) have found that Peterson's Rational Learning Problem increases in difficulty disproportionately to the increase in the length of the problem. Cook (21, 22) used in one experiment addition and subtraction problems composed of different numbers of digits and presented them orally. While there was little difference in the number of trials or the amount of time required to solve the 2 and 3 digit problems, a comparison of the results for the 3, 4, 5 and 6 digit problems showed an increase in difficulty disproportionate to the increase in the number of arithmetical operations involved. A second experiment employed as material a series of Peterson's disc puzzles of different lengths and the results confirmed those of the first experiment. These results are in general agreement with those obtained in the analogous problem in memorization.

Character of the Material. Wilkins (107) investigated the effect of changed material on ability to do syllogistic reasoning. She found that syllogisms were more easily solved correctly when the terms of the syllogisms were familiar than when they were unfamiliar or symbolic. Heidbreder (40) found that concepts were more easily evolved from pictured than from verbal material; and that in pictured material, concepts based on concrete objects were mastered with least difficulty, those based on number with greatest difficulty.

Caffeine. The problem of the influence of drugs upon problem-solving and upon thinking in general seems to present an interesting and fruitful line of research, but, so far as the writers know, only one experiment has been performed upon this problem. Holck (**43**), using himself as subject, compared the number of two-move chess problems solved after injections containing 200 milligrams of caffeine sodium benzoate with the number solved after injections of saline solution. The subject did not know which injection he was receiving. The results showed that there was no significant difference in efficiency under the 2 conditions.

Age. The studies of problem-solving in preschool children (*e.g.* Alpert, **4**, Harter, **34**, Matheson, **64**, and Roberts, **84**) all report that there is some relationship between age, especially mental age, and achievement. The correlations of achievement with MA range from only +.27 to +.42, but the tendency for a positive relationship is uniformly found. Maier (**63**) concluded from his experiments that reasoning ability, as he defines it, matures later than learning ability; its time of appearance varies with the child and is related to mental age. Heidbreder's (**37**, **38**) results offer further evidence that problem-solving ability increases with age. Her subjects included groups of children from 2½ to 10 years of age and a group of adults. An interesting feature of her study lies in the analysis made of reasons given for the choices made in multiple choice situations. Although there was considerable overlapping, the distribution of reasons formed, in general, a "maturity scale."

Extensive investigations of syllogistic reasoning in children from 7 to 17 years of age have been made by Müller (**69**, **70**, **71**). The greatest increases in the ability to make inferences were found between the first and second and the second and third school years. Müller also found that the tendency to imagine concrete situations to represent the premises diminishes with increasing age.

Sells (**88**) studied the relation of age to scores on his syllogism test (administered as a power test) in a group of adults ranging from 20 to 70 years of age. No relationship of age to test score was found; the correlation obtained was −.196.

Sex. Chou, Chen, and Chao (**15**) gave Thurstone's Reasoning Test B to groups of college students. The sex differences in amount of time required to judge a problem and in number of errors made were insignificant. The only difference lay in the very slight tendency of men to judge correct conclusions as wrong while women tended to judge wrong conclusions as correct. The authors con-

cluded that the test employed was too easy to differentiate subjects of college age. Arons (**5**) found that men were superior to women on a Yerkes multiple choice problem and believed that this difference was not attributable to a sampling error. Billings (**8**), using problems from a variety of academic fields, reported that while men ranked but little higher than women on the Army Alpha and on information tests their score on the problems was 48.7% higher than that of the women. Maier (**62**), using both practical mechanical problems and ingenuity problems, found a distinct superiority of men over women. He suggested that the sex difference may be real or that it may mean the problems were better suited to the men. In these experiments, Maier also found that the women profited more than the men from suggestions given them to avoid habitual modes of thinking and to look for new methods of attack.

IV. Critique of Methods

Although it is impossible to summarize the results of these experiments in a few general conclusions, it is possible to make some evaluation of the methods which have been employed to study thinking. Less ingenuity in inventing methods seems to have been exerted in this field than in almost any other. The first studies of thinking done in the laboratory consisted of giving the subject simple problems to solve (such as controlled associations) and asking him to introspect upon his thought processes. This was sometimes called the method of "systematic self-observation." The data consisted of voluminous protocols from which it was almost impossible to derive any quantitative analysis. This method was fruitful as a way of exploring a new field, but now, 35 years later, we might expect that experiments of a critical nature with more advanced techniques should prevail. Yet, to make a moderate estimate, one-half of the studies quoted have employed the same essential procedure. Sometimes it is varied slightly by asking the subject to "think aloud" as he solves the problem (Claparède's "*réflexion parlée*"), or children are used as subjects and their behavior and spontaneous remarks recorded. The situation is reflected in Lindworsky's (**58**) summary of methods of investigating thinking, where no method except that of self-observation or its variants is even suggested.

A fair number of studies have varied some condition systematically and measured its effects on resultant behavior. Instructions have been varied, emotional stimuli introduced, drugs administered, and so forth, and the effects on the number and quality of solutions

have been observed and compared with control conditions. A related technique which has been occasionally employed is that of selecting groups of subjects for some characteristic, such as age or personality type, and comparing their performance in a problem situation with that of a control group. Other techniques which border on experimental method are questionnaire studies and elaborate statistical analyses. Both of these have been used with some profit in the investigations quoted above.

The methods mentioned in the last paragraph represent some advance over the hackneyed method of *Selbstbeobachtung*. But the researches might, on the whole, be characterized as unsystematic. There have been amazingly few attempts to formulate an hypothesis and test it in carefully planned experiments. There should be a great revival of interest in the psychology of thinking when investigations in the field become truly critical experiments.

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FACTORS CONDITIONING MOTOR SPEED AND TEMPO[1]

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The literature relating to the problem of motor speed is highly scattered, and has never been systematically reviewed. Most of the investigations have been primarily of either "practical" or merely "factual" interest. Studies of the former type, for example, have sought to analyze the tempo of various occupational workers with the aim of scientifically reducing the time consumed and otherwise improving precision and efficiency. Others have sought to evaluate the techniques of job analysis in relation to improved methods of selecting personnel for motor skills. The latter or mere "factual" type of study has been interested primarily in the speed, tempo, or other temporal characteristics of a given individual or group *per se*. Thus attempts have been made to trace "the" motor development of the child, to investigate existing sex differences, or to establish alleged norms or upper limits of motor functions. All of such investigations may be contrasted with a third type in which the dominant interest is to determine the *factors responsible for the manifested motor speed*. The speed or tempo of response itself, whatever it may be in a given case, is thus of importance only in so far as it is the inevitable outcome of certain underlying factors.[2] The study of results pertaining to certain of these alleged factors is the province of the present paper.

It seemed desirable, in the present review, to attempt to classify the material with respect to certain broad categories. Consequently, the investigations have been grouped under 9 major headings, each representing a different approach to the factors which might condition motor speed or tempo. These categories, in the order of their consideration, are: *Physiological Factors, Age or Maturational Factors, Sex Differences, "Constitutional" Type, "Racial" Type,*

[1] The writer is indebted to Professor Franz Boas of the Department of Anthropology, Columbia University, for his interest and coöperation in the present paper.

[2] For a theoretical discussion of this approach to problems of general psychology and anthropology, the reader is referred to Foley (35, 36).

Intercorrelational Analysis, Factor Pattern Analysis, "Character" Type ("Personal Tempo"), and *Social and Occupational Factors.* It should be kept in mind that such a classification is in large measure arbitrary and leaves much to be desired. Its main justification seems to be that of convenience. Many of these rubrics overlap, and frequently the same investigation presents data pertinent to more than one heading, in which case an attempt has been made to place the study in that group to which it seems most relevant.

It will be noted that certain of these investigations have dealt with natural, convenient, or optimal speed of motor performance in addition to maximal speed. The former problem has been overlooked by many investigators, although from both a theoretical and a technological point of view its study is certainly justified. In a paper presented before the British Association for the Advancement of Science in 1927, Mace (**72**), for example, has called attention to the importance of a consideration of both problems in the hope that such work might help in the elucidation of the distinction between "abilities" and "temperamental traits." Mace has pointed out that the ratio of natural to maximal rates of work is far from constant; hence it would seem important to standardize mental tests for both rates.

The plurality of factors which might influence or determine the speed of motor reactions has been recognized by Downey (**25**), Whipple (**106**), and others. In attempting to secure an index symptomatic of general bodily speed of movement, Downey (**25**) has made use of graphic speed. She states (p. 88) that "there are, however, several limitations on the use of handwriting in getting at general speed of movement, which should be kept in mind," and lists as important such factors as physical condition, age, practice, social and professional differences, and habit conflicts. Referring to the results of tapping experiments, Whipple (**106**, p. 138) states that "the maximum rate of voluntary movement varies with the individual, with sex, with maturity, with the side of the body used, with practice, with the number of trials (duration of the experiment), with fatigue, with mental excitement, with the time of the day, but not, within wide limits, with the amplitude of the movement." He also discusses the possible influences of general physical condition, mental disability, and social status.

It is obvious, of course, that certain factors in the immediate experimental situation, such as the mental attitude or *Einstellung* on the part of the subject as well as the particular characteristics of the task to be performed, will also condition the speed and rhythm of

motor performance. Liberson (**68**), for example, found that in tasks requiring alternate flexion and extension of the finger at maximal speed, rhythm depended upon the directions given to the subject with respect to the movement to be performed, although the same muscles were involved in both conditions. The form of the rhythmic movement also varied with the "mental attitude," which in addition diminished the movements when the subject, instead of making free extension movements, was required to strike against a button placed dorsal to the hand. In this case the maximal movement was more rapid. Wachholder (**103**) likewise found a relation between the self-chosen speed and amplitude of movements of the hand or lower arm. Rhythms were most rapid under his particular conditions when the amplitude was relatively large. Our present interest, however, is not in data which are the result of arbitrarily imposed differences in the experimental situation *per se*, but rather in certain possible underlying factors which condition the temporal characteristics of motor response in any situation in which it may occur.

Physiological Factors

Let us first consider certain of the findings regarding possible physiological factors in the speed of tapping and other simple motor functions. The tapping test, as Whipple (**106**, p. 130) has pointed out, has been more frequently used than any other motor test, although "we do not know precisely what may be the ultimate neural or psycho-physical factors that condition the rate." Wells (**105**, p. 444) also states:

> "What is the precise physiological significance of the maximum rate is by no means well made out. . . . It seems to be generally conceded that it is limited by the refractory phase of the synapses in the motor pathways, but that does not make the tapping test a measure of the period of this refractory phase; at least, not in the earlier stages of practice. . . . In the beginning, as we ordinarily have to apply the test, the factors in speed are probably those of coördination mainly, and cannot be expected to afford information about the condition of the motor pathways as given in the refractory phase."

Dresslar (**26**) has reported that a vigorous walk decreases the rate for rapid voluntary movements of the wrist (tapping on Morse key), and concludes that increased "central" activity favors increased rapidity of voluntary movements. This finding is not dissimilar to certain of the more recent results on "irradiation," although the exact nature of the physiological mechanism in such cases awaits empirical demonstration. Ching (**21**) has contended that the factor

limiting the speed of up and down movements of the human finger is the central nervous system, although no actual evidence for such a statement is presented. Thus it would seem that the available data do not justify the consideration of maximal (or optimal) speed of tapping or any other simple motor response as a primary function of physiological mechanisms. We shall further discuss certain physiological findings under related topics below.

Although dealing with motor steadiness rather than speed, a recent study by Zenkevich (**110**) of the morphological structure of the wrist in adolescent workers is perhaps indirectly relevant to the general problem of physiological factors conditioning motor ability. Zenkevich examined apprentices to turners and locksmiths (N=270 and 45 respectively), aged 15–19, comparing the morphological type of the wrist with the motor ability, obtained by means of Morse's apparatus, consisting of a support and tremometer. The results indicated a positive, although not perfect, relation between breadth of wrist and motor index. No attempt was made to determine the effect of trade experience on either or both of these variables.

Age or Maturational Factors

Several attempts have been made to investigate age or maturational factors in speed of motor behavior. The studies of Goodenough and Tinker (**48**), Goodenough (**46**), Bayley (**6**), and Oates (**82**) are typical. In the first mentioned study (**48**), comparison of the various tapping speeds of kindergarten children and adults revealed a developmental sequence from large to small muscle groups. The study by Goodenough (**46**), employing a group of 240 children ranging in age from 2½ to 5½ years, included speed of tapping with both stylus and forefinger. The growth curves for both methods showed negative acceleration, and confirmed the previous findings of functional maturity from large to small muscle groups in comparison with adult norms in the tapping tests on index, middle, and little fingers. Correlations among scores by different methods were generally low and positive. Individual differences persisted over several months, but tended to become less consistent after 2 years. "Laterality of function, while present even in early childhood, becomes distinctly modified by differential practice as age advances" (p. 319).

Bayley (**6**) tested 61 white infants repeatedly from birth to 3 years of age by means of the California infant scale of motor development. Among the other findings, Bayley reports a gradual

increase in functional independence of motor and intellectual abilities as maturation proceeds, and contends that motor abilities seem to be more closely related to each other in infants than in adults. It is obvious that environmental-stimulational factors were by no means eliminated in these investigations, so that the resulting changes cannot be attributed to mere chronological or maturational factors. In fact, the increasing functional independence of various traits and the functional development from large- to small-muscle groups might readily be envisaged in terms of the ever increasing complexity and differentiation of the institutionalized child-in-environment situation. Goodenough herself attributes the modified laterality of function to differential practice, and it would seem that this factor might also account for the various other results obtained.

An entirely different approach to the problem of age or maturational factors has been made by Oates (**82**) who gave the "speed of movement" (natural or convenient speed) and the "ability to speed" (maximal speed) tests from the Downey-Uhrbrock nonverbal Will-Temperament Test (cf. **25**) to 286 boys aged 11 to 18. The average correlation between "ability to speed" and intelligence test scores was —.175, while the correlation between the former measure and age was —.595. From a consideration of these results in terms of what is called the "biological significance of intelligence tests," Oates has suggested, among other things, that efficiency of reaction, as measured by "ability to speed," reaches maturity early, whereas efficiency of meaning, as represented in intelligence test performance, matures later. The data are interpreted as emphasizing "the necessity for analyzing the activities involved in intelligence tests from the point of view of the functional maturity of the processes underlying them." Again, as in the above studies, no control has been made of stimulational factors in the reactional biography of the child, so that there is no empirical justification for attributing any of such test performances to the operation of alleged maturational factors.

The development of rhythm, a relevant although somewhat different and perhaps more complicated motor trait, has been investigated in young children by Jersild and Bienstock (**50**). The growth of ability to keep time to music and the effect of various factors on accuracy in keeping time were studied. The subjects were 94 children aged 2 to 5 years, and 17 adults. Cinematic records were obtained when the subject walked or beat time with his hands to the accompaniment of piano music; the film also included a record of

accented beats of the music, as shown by a light governed by perforations in the music roll, and of the passage of time. Average scores rose substantially from 2 to 5 years, and adults scored about twice as high as five-year-olds. "Practice," in the form of 40 to 50 opportunities to keep time to the music, brought an increase in score, but not as large as the scores of "unpracticed" older children. The terminology here, of course, is definitely misleading, since the so-called "unpracticed" older children had unquestionably had much more relevant, though indirect, stimulation and practice than their younger and so-called "practiced" rivals. The frequency with which rhythm-producing situations are encountered in the everyday life of most children is well known to everyone.

Sex Differences

The problem of sex differences in tapping and simple motor speeds has been summarized by Wooley (**108**) and by Whipple (**106**). The former (p. 356) draws the following tentative generalizations from the available data:

> (1) "Girls develop faster than boys from infancy"; (2) "boys are superior to girls in rapidity of movement under conditions in which the direction of attention remains fixed, as in reaction time and tapping"; and (3) "girls are superior to boys in rapidity of motion in types of activity in which the direction of attention is constantly shifting—activities which involve rapid adaptations—such as card sorting, mirror drawing, reading and writing."

With regard to tapping speed, Whipple (**106**, pp. 139–140) also states:

> "The results of most investigators lead to the conclusion that boys are faster than girls, and that this sex difference increases with age. Thus, Miss Thompson, who worked with a small number of adults, found that 88% of men exceeded the median speed of women. Similarly, Burt and Moore place the median for boys at 86.5 and for girls at 80.5 taps in 15 sec., and find that 69.8% of boys exceed the median speed of girls. Bolton, however, has reported that 'the girls are uniformly better than the boys,' while Bryan found girls superior at 13, when they showed improvement and the boys little or none—a tendency that is apparently allied to the actual crossing of the curves of height and weight. More elaborate experiments upon adults (10 men and 10 women) by Wells now seem to indicate that women surpass men in tapping with the right hand in the first experiment, whereas elsewhere they are inferior: the sex differences found by this investigator are said to be 'mainly in those features of the experiment which especially involve the affective factor in the subject's attitude, and they are manifestations of the greater responsiveness of the women to this affective element.'"

Accepting as authentic the usual results regarding the slight superiority of boys over girls in speed of tapping, we still do not know whether this difference is really owing to the different physiologico-anatomical makeup of the two sexes, or whether it is the result of stimulational and experiential factors incumbent upon the vastly different institutionalized status and training which each receives in our society. The fact that the sex difference increases with age would seem to lend support to the latter hypothesis. There is need here either for an experimentally controlled genetic study beginning at birth if not sooner or for comparative data from other cultures in which the conventionalized stimulation imposed upon the two sexes is different from, or even opposite to, that prevalent in our own culture (cf. Foley, **35**). What we know of such cultural groups would make us anticipate quite different results from those obtained in our particular society.

In certain other simple motor traits, sex differences in our society are negligible. Whipple (**106**) reports that in aiming, "sex differences are slight, but, on the whole, boys are more accurate than girls, and men than women" (p. 151). In tracing or precision, "with either hand, boys are probably slightly superior to girls . . . and men to women" (p. 155). In steadiness or involuntary movement, "distinct sex differences have not been established" (p. 159). A recent experiment by Husband and Ludden (**49**), employing pursuit rotor, spool packing, serial discrimeter, crank drill, and speed of tapping tests from the Seashore Motor Skills Unit, showed no particular or consistent differences between 30 men and 40 women. They conclude (p. 422): "No particular types of operation on which one sex or the other might be consistently superior were suggested by our evidence. . . . All this evidence points in general to a practical equality of the two sexes in the motor abilities tested."

"Constitutional" Type

Another related physiologico-anatomical approach to the general question of speed is that attempting to link motor characteristics with constitutional types. In a representative study, Enke (**30**) investigated the relation between motor peculiarities and bodily morphology in 500 male and female subjects, aged 16 to 65, of all degrees of "intelligence" and training. Preliminary structural diagnoses, obtained by means of the Kretschmer psycho-biogram, were followed by a series of tests designed to elicit the muscular differences among normal and psychotic constitutional varieties. The average tapping

rate per 30 seconds was 38.5 for pyknics, 68.3 for athletics, and 84.3 for leptosomes. Counting in time with a metronome was easiest for the pyknics and hardest for the leptosomes. In a steadiness test, the descending order of excellence for both speed and accuracy was: leptosomes, pyknics, athletics. Thus with regard to speed Enke concludes that pyknics exhibit a slower tempo and a more irregular pattern of movement than the other types.

In summarizing the traditional literature in this field, Klineberg, Asch, and Block (**58**) state that on the motor side, leptosomes are reported to tap faster when told to tap at their natural or most comfortable rate (Van der Horst, **102**; Enke, **31**). In motor activities in which many muscles come into play, pyknics are reported superior; their movements are more rounded and flowing (Enke, **30**, **31**; Enke and Meerowitsch, **32**; Oseretsky, **83**; Ossipowa, **84**). Very fine and delicate movements are, however, allegedly carried out better by leptosomes.

Van der Horst (**102**) reported, for example, that his normal and psychotic leptosomes ranged between 19 and 27 optimal taps per ten-second period, while the normal and psychotic pyknics ranged between 9 and 12. The pyknic group consistently tapped at a slower rate than the leptosomes, the only exception being in the case of 3 manic-depressive subjects in the manic phase. Schiassi (**93**) has also attempted a motor biotypology derived from combinations of quantitative and qualitative motor characteristics.

In an attempt to investigate the validity of Van der Horst's contentions, Mohr and Gundlach (**79**) conducted an intensive quantitative investigation on a group of male convicts in the Illinois State Prison.

> A total of 600 men were measured, out of which 89 were selected as good representatives of leptosomes (asthenics), athletics, and pyknics, on the basis of Kretschmerian anthropometric measures and diagnosis. Among the large number of tests given were speed of tapping and of writing, visual reaction time, and cancellation. None of these showed a reliable difference between any 2 of the 3 groups with the exception of the leptosome-pyknic difference in maximal speed of writing "United States of America." This particular difference, however, was complicated by the fact that more of the pyknics were unable to spell the words unassisted, as well as by the general fact that the pyknic group was more than 6 years older than the leptosome.

With regard to the tapping experiment in particular, Mohr and Gundlach state (p. 136) that "the results show that there is a very slight difference in the initial tapping rate between the asthenic and the pyknic groups, which has practically no significance in view of

the deviation of the two groups." They concluded, moreover, that " no definite tapping rate is demonstrated . . . contrary to the definite results reported by Van der Horst." None of their subjects tapped at the low rate reported by Van der Horst to be typical for the pyknic group. With regard to speed of writing, their results also showed " slight and unreliable differences in ' psychic tempo ' among the three groups."

Many of these studies are open to serious criticism. Aside from the questionable existence of so-called " constitutional types," there are a number of inadequate or uncontrolled variables in most of such experiments. The groups employed are frequently small. Averages are often reported with no indication of intra-group variability or inter-group overlapping. Quantitative data are frequently lacking and only descriptive observations reported. The tests are often inadequate or poorly standardized. The groups themselves, selected chiefly on the basis of " constitutional type," usually differ in other essential respects. Not only may the groups differ in sex, but the pyknics are commonly older than the leptosomes, and this age difference could readily account for the observed speed differences. Little or no attempt has been made by most investigators to control this factor. Social and cultural background may also have frequently affected the results, leptosomes, for example, being more commonly found among the higher social and educational levels. Thus it seems that the positive data on the relation of speed to constitutional type can readily be explained on the basis of other experimentally uncontrolled factors.

The actual absence of any such relation between motor speed and " constitutional type " has been clearly demonstrated in a recent study by Klineberg, Asch, and Block (**58**). The subjects were 153 male college students, all from the same institution. The average age was 19 years 9 months, and there was no marked age variation within the group. Among the tests administered were four-letter cancellation and three-number cancellation. No reliable differences were found between leptosome and pyknic groups, the critical ratios ranging from 0.45 to 1.54.

" Racial " Type

A still further biologically related line of approach has been through " race," many writers contending that there are racially conditioned differences in speed of movement. Klineberg (**57**, Chapters VII and VIII) has summarized the more important literature. An

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early study by Bache (**4**) on the auditory, visual, and electrical reaction time of 12 Whites, 11 Indians, and 11 Negroes showed the Whites to be the slowest and the Indians fastest on each of the 3 measures. Finding it necessary to explain why a presumably "superior" group should do so poorly, Bache states that it is the "lower," not the "higher" man, who is more responsive to such stimuli, which are related to secondary reflex action; men, in proportion to their intellectuality, tend less and less to quickness of response in the automatic sphere, the reflective man being the slower being.

Lapicque (**64**) tested the visual reaction time of 28 Europeans, 18 Andamanese, and 7 Hindus. Unlike Bache, he found the Europeans to be the quickest. The variability was so large, however, corresponding to differences in social level, that Lapicque was inclined to believe that race is of little importance.

Myers (**80**), who studied reaction time as part of the work of the Cambridge expedition to Torres Straits in 1899, gave tests to natives of Murray Island and Sarawak, and compared the results with those obtained in England. The groups ranged in size from 17 to 24 in auditory reaction tests and from 9 to 21 in tests of visual reaction. In general, there was no consistent difference between the Murray Islanders and the English group, each being superior in one of the experiments. The natives of Sarawak, however, were fastest of the 3 groups in both. In choice-visual reaction time, the Murray Islanders were inferior to the English, although Myers points out that the latter were all of the well-educated class and that a fairer comparison would have been between the Murray Islanders and English villagers. Thus, as Klineberg (**57**, p. 141) points out, "there is no doubt that here, as in the more complicated psychological measures, social and economic factors must be considered in all racial comparisons."

A recent study in Hawaii by Livesay and Louttit (**71**) involved the testing of 286 university students of different racial and national origin. Indices of visual, auditory, and choice reaction times were secured with groups ranging in size from 14 to 69. The following "racial" groups were employed: Caucasian, Chinese, Japanese, and Part-Hawaiian. Males and females were classified separately. The investigators conclude that race differences in reaction time are small and insignificant, and do not consistently favor any one group over any other. The sex differences, though also small, are more consistent than the race differences and favor the male subjects.

Many of the various "racial" differences reported on "intelligence" tests have, on closer inspection, seemed to be cultural or national rather than "racial." As Klineberg (57) has pointed out, the attitude toward speed varies greatly in different cultures, and in the case of most intelligence tests, the scores are markedly affected by the rate at which the questions are answered or the manual tasks performed. A premium is placed on speed in western, and particularly in American, civilization. Children soon learn to work quickly at their tasks, even when there is no special instruction to hurry. Klineberg (57, p. 160) contrasts this attitude with that prevailing in other communities, such as the American Indians, for example, where children are specifically taught to think carefully before they act and not to answer before they are sure of what they say. Peterson and his associates (86) have noted the relative indifference to speed among Negroes, and Klineberg (56) found that the injunction to "do this as quickly as you can" seemed to make no impression whatsoever on American Indian children on the Yakima reservation. Porteus (87) has made the same point regarding his Australian subjects; and this general finding would undoubtedly hold true for many of the Oriental culture patterns.

Peterson, Lanier, and Walker (86), using the authors' rational learning, mental maze, and disc transfer tests, designed to eliminate differences in ability due to education or attitude toward speed and accuracy, found that White children excelled Negro children in speed and in reasoning, but that the Negroes equalled the Whites in retention and memory. In eye-muscle coördination speed tests, the Whites excelled.

Klineberg (55) conducted a study on 120 full-blooded or almost full-blooded children on the Yakima Indian Reservation, in the State of Washington, and 110 White children living in the town of Toppenish, in the heart of the reservation.

The tests administered included the Casuist Form Board, Mare and Foal, Healy A, Triangle, and Knox Cube Tests. He found a striking qualitative difference in the way the 2 groups worked. Typically, the White child jumped at the task, tried out a piece quickly in a succession of positions until reaching the correct one, and finished the test rapidly but with a relatively large number of errors. The Indian child, though he received the same instructions, attacked the problem very slowly and cautiously, picking up one piece and placing it correctly before proceeding to the next. He usually took much longer than the White child, but made fewer errors. Thus Klineberg maintained that the use of speed as a test of excellence of performance, with difficulty kept constant,

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may be entirely unfair to some groups in which the speed factor does not have the significance habitually ascribed to it. The White's emphasis on speed had not penetrated the culture pattern of the Indian.

In a further and more extensive elaboration of this experiment, Klineberg (**56**) gave the Pintner-Paterson series of tests to Indian groups in 3 localities and to Negro groups in 6 localities, all within the United States. The results, confirming the former study, showed the White superior to the Indian and Negro children in *time* rather than in *accuracy*. The greater speed of the White children seemed to be determined by environmental rather than by "racial" differences. Thus speed should also be included among the non-racial factors which have given experimental support to the theory of "racial" differences. Among the other non-racial factors, Klineberg mentions language, schooling, culture, social and economic status, rapport, motivation, and sampling.

Klineberg (**57**, p. 159) contends that "this indifference to speed is cultural and not innate." His review of the material bearing upon basal metabolism and other fundamental organic functions "revealed no consistent physiological basis for a racial difference in speed of reaction." He adds that "the further fact that American Indian children who have lived a long time among Whites, or who attend a busy and progressive school, show a definite tendency to approximate White behavior in this respect, also points in the same direction" (**57**, *cf.* **56**).

Lambeth and Lanier (**61**) have attempted further to analyze experimentally Klineberg's view that racial differences found on many psychological tests are essentially differences of speed. The Stanford-Binet scale, a rational learning test, and 6 tests of rate of response were given to 30 White and to 30 Negro twelve-year-old boys. Comparisons showed that the more complex the test, the higher the relative score made by the Whites; and, since there was roughly a parallel increase of speed difference in their favor, Lambeth and Lanier maintain that the theory of a general speed difference between the races is untenable. Since the intercorrelations between the various tests were found to be higher for the Negroes, they conclude that this is possibly indicative of a more "primitive" and less "differentiated" type of mental organization. For a critical evaluation of such antiquated mentalistic views, the reader is referred to Boas (**10**), Klineberg (**57**), and Radin (**88**).

Intercorrelational Analysis

We now come to what may be termed the statistical approach to motor speed. We shall first consider certain of those studies which have attempted some form of intercorrelational analysis. The primary aim of such investigations is to discover if speed, tempo, or other motor characteristic is "specific"—showing low correlation with such attributes of other reactions, or whether such correlations are high and positive, in which case the several manifestations may be attributed to a common or "general" factor. In each of the studies in this section, no techniques beyond correlational analysis were employed.

Although our immediate interest is in the question of motor speed or tempo, certain other studies are relevant in so far as they have investigated the interrelationship of motor abilities, or the relationship of motor speed to "intelligence" or complex mental abilities. Although a classification of these studies into definite sub-categories would make the exposition clearer, it will not be attempted, since all do not fall into clear-cut groups, many of them being more or less "transitional" and presenting data on several of such issues. In general, however, the trend of our discussion will take the following form: After noting the broad generalizations and conclusions which have been reached by previous investigators who have surveyed certain portions of the field, we shall report, without definite demarcation, the more important and relevant experiments dealing with (a) the correlation between different sensori-motor traits—evidence regarding the existence or non-existence of a general "motor factor," (b) the correlation between different sensori-motor *speed* tests—evidence regarding the existence or non-existence of a general "motor speed factor," and (c) the correlation between "motor speed" and "complex mental abilities."

McFarland (75) has surveyed much of the literature connected with "the rôle of speed in mental ability." He has considered such investigations under the following categories: (a) "investigations in which the speed of reaction has been studied in connection with sensation and perception or in the simpler psychological processes," (b) "those investigations in which speed of reaction has been related to the higher mental processes or some factor in mental ability such as attention, imagination, or memory," and (c) "those investigations dealing with rate of response in connection with general intelligence or with a specific ability." McFarland has concluded (p. 608) that

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"probably the chief feature of this review . . . is the fact that the evidence is not clearly defined either for or against the existence of general speed ability."

Other surveys of portions of this literature have been made by Beck (7), Whipple (106), Allport and Vernon (2), and Tinker (100). Whipple, for example, after pointing out that tapping has been thought to afford a better motor index than any other single test, has stated (p. 130) that "recent work with tapping, . . . while not discouraging the belief that the test has value, has shown that we cannot regard speed of voluntary movement as an unequivocal and comprehensive 'index of voluntary ability,' because a high gross rate does not necessarily go hand in hand with high speed in other phases of motor response."

The conclusion of Whipple in favor of specificity of motor speed has been substantiated by Tinker (100) with respect to the relation between speed of response and mental ability. From a survey of the available literature, Tinker concludes that no common factor runs through both motor and mental test responses, but that speed is related to mental and scholastic test responses when speed and ability are measured on the same kind of material. We shall cite Tinker's specific conclusions in detail in subsequent discussion of experimental results. Campbell (20, p. 612) has likewise concluded that "generality of motor abilities is not supported by experimental findings. . . . A correlation coefficient of +.25 may be taken as representative of these data. 'Specificity in motor abilities is therefore the favored explanatory principle."

Among the earliest studies dealing with the correlation between different sensori-motor traits was that of Wissler (107), who, following the pioneer work of Cattell, tested 250 freshmen and a small number of seniors in Columbia College, in addition to a limited number of Barnard College women. The measures employed were divided into 4 general categories: (a) quickness and accuracy, (b) memory, (c) physical tests, and (d) class standing.

More specifically, the tests included: perception of size, size of head, strength of hand, fatigue, eyesight, color vision, hearing, perception of pitch, perception of weight or force of movement, sensation areas, sensitiveness to pain, color preference, association, imagery, memory (auditory, visual, logical, retrospective), reaction time, rate of perception, color naming, rhythm and perception of time, accuracy of movement, and rate of movement.

The last named test, which is most relevant to our present interest, consisted of a blank ruled into 100 one-cm. squares, into

each of which the subject was to put a dot. The results showed low intercorrelations between all of the various tests. The following data are typical:

Reaction time and marking out A's	r=—0.05 (252 cases)
Reaction time and color naming	r=+0.15 (118 cases)
Reaction time and association time	r=+0.08 (153 cases)
Reaction time and movement time	r=+0.14 (90 cases)
Color naming and marking out A's	r=+0.21 (159 cases)
Color naming and movement time	r=+0.19 (97 cases)

Wissler states (p. 62): "The general conclusions are: 1. That the laboratory mental tests show little intercorrelation in the case of college students. 2. That the physical tests show a general tendency to correlate among themselves but only a very slight degree with the mental tests. 3. That the markings of students in college classes correlate with themselves to a considerable degree but not with the tests made in the laboratory."

Wissler's results have been corroborated by Perrin (**85**). The latter employed the Bogardus and card sorting tests of complex motor ability as well as 14 tests of simple motor traits, such as reaction time, inhibition, motor memory, weight discrimination, aiming, balancing, rhythm, tapping, steadiness, tracing, etc. The subjects were 51 University of Texas undergraduates. None of the obtained correlations was significant, from which Perrin concludes (p. 49): "It seems quite patent that motor ability is not general, but that it is somewhat definitely specialized."

Similar results favoring the specificity of motor functions were obtained by Braunshausen (**14**), who tested 16 students in a University Institute of Education using a wide variety of motor tests. The correlations, calculated according to the Spearman formula, between the different series of measurements and (for rapidity of movements) between the total scores and the rapidity of tracing dots, circles, etc., were generally rather low (less than .60). The highest was .86, between total speed and speed in tracing circles. Certain negative correlations were found: kinesthetic memory and steadiness, —0.9; and rapidity and steadiness, —0.3. The correlation between general motor ability and intelligence, as measured by the Otis-Duthil scale, was 0.3.

Considering the more immediate problem of motor *speed*, we find a certain disagreement in experimental results. We shall first consider those results favoring a "general factor" interpretation. Courthial, Van de Stadt, and Claparède (**22**), investigating the

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problem of speed, quality, and duration of individual performance, conclude from a preliminary survey of the literature that there is no general agreement among the various investigators, although they maintain that for a given individual the quality of the work is much more variable than the speed.

In their own experiment, 2 tests were used: (1) *carré de chiffres,* involving the substitution of letters under two-place numbers, in order, after locating the numbers, and (2) arithmetic computation, including addition, subtraction, multiplication, and division. The subjects were 77 Normal School students, 54 boys aged 16–20, and 23 girls aged 19–21. The reliability coefficient from the first and second form of the same test was .75 for the *carré de chiffres;* the arithmetic computation also yielded high positive correlations within each of the 4 operations. Correlations between 2 kinds of speed tests were much lower, the authors stating (p. 221): *"La constance de la vitesse baisse considerablement lorsqu'il s'agit de deux épreuves différentes, telles les tests d'arithmétique et l'épreuve du carré de chiffres."*

It will be noted that in this investigation, as in most of the others to be reported, no attempt was made to utilize subjects who, because of vocational or other habitual factors, might be expected to manifest wide differences in the motor abilities under consideration. It is only in such cases that the most clear-cut evidence regarding specificity of motor speed or tempo would be logically anticipated.

Gilbert (**45**) has also dealt with the constancy of speed in the performance of various tasks, and has favored an interpretation based on a "general" factor.

The experiment consisted of 2 parts, the first being a laboratory study with 4 observers, and the second an individual group study of 16 girls between the ages of 9 and 13. In each of 12 tests employed, the items were presented and timed separately and only those which were correct were included in the results. Accuracy was thus kept constant and speed, to this extent, isolated. In summarizing the results, Gilbert states that for the 4 observers in the laboratory study the tendency for positive correlations based on the ranks would seem to support the theory that speed is a constant factor. She points out that the reliability of the ranking obviously depends upon the differences between the means which she finds to be in the most part significant.

Gilbert concludes by stating that "it seems evident that for the different tasks set speed relationships within a group are relatively stable. To the extent that these tests are representative of intellectual performance in general it may be concluded that the person who is quick in one task will be quick in others also."

Campbell (**18**), in a study of the "personal equation" in serial pursuit performances, employed as subjects 50 sophomores at the University of Oregon.

A baton was strapped to each hand and foot of the subject; the task involved a response to a light by striking a metal plate or key through an octagonal aperture in a continuous sequence. The mean score of correct responses was 5% smaller (significant differences) for the legs than for the arms. The average correlation between scores for legs and arms was .84±.03. Right and left scores for arms and legs correlated respectively .98±.01 and .95±.01.

Campbell concludes (p. 792) that "under these conditions and where speed alone was emphasized in the psychological performances, the existence of a high degree of 'general' motor ability seems to have been demonstrated." In another study, designed to introduce several controls for the purpose of determining to what extent the general ability found was due to the use of a particular sense organ, Campbell (19) concluded that the absence of a general motor ability, whenever found, is due not so much to the simple difference between performances in sense organs used as to the large difference apparent in the cognitive processes of discrimination necessary for the performances.

A further study by Campbell (20) also involved analysis of the function of the various bodily members in motor performance.

Four tasks, known to be highly correlated, were varied and repeated in such a way as to indicate the extent to which each of 3 factors entering into the performance contributed to the obtained high degree of correlation. These 3 factors were: (1) the limb employed to do the task, whether arm or leg; (2) the rightness or leftness of the limb used; and (3) the visibility or non-visibility of the reaction keys. Six tasks in all were performed by each of 54 male subjects. The results seemed to indicate that generality of ability, as indicated by correlation between performances, was greater on the average if in both of the performances correlated an arm or a leg was used, and less if in one of the performances correlated an arm was used and in the other a leg. Generality of ability was about the same if in both of the performances correlated a right or a left limb was used, or if in one performance a right limb was used and in the other a left. Generality of ability was the same if in both performances correlated the reaction keys were visible or invisible, or if in one performance the keys were visible and in the other invisible. Generality of ability was still present to a fairly high degree when all 3 of the factors investigated varied simultaneously between correlated performances.

An earlier experiment by Gates and Scott (44), comprising a study of motor speed and dexterity among young children, also included an analysis of the data with respect to different bodily members.

Fifty children, most of whom were 4½ to 6 years of age, were given 17 individual tests of speed in motor functions: dropping marbles through a slot with right hand, left hand, and both hands; tapping bell with right, left, and

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alternately; sorting cards with right, with left, etc. Age, intelligence scores, and teachers' ratings were also secured. Statistical treatment showed no closer correlation between motor dexterity and intelligence in children than in adults. Intercorrelations of the tests suggest that motor speed is highly specialized in about the same degree in young children as in adults. Correlations tend to be high when the performances are of the same type though executed by different members, when they are executed by the same members, or when they involve the same side of the body.

We come now to certain studies which have aimed especially to investigate the problem of intercorrelation between the speeds of various exclusively motor reactions. Certain of these experiments, particularly those concerning the speed and pressure aspects of tapping, have been reviewed by Allport and Vernon (**2**) in their comprehensive "Studies in Expressive Movement." Braun (**13**) found that the normal, *optimal*,[3] or "most convenient" speed of tapping with the right hand correlated more highly with 7 other measures of speed of normal movement than any other single measure, giving an average correlation of .534. He found, moreover, that this speed possessed a very high reliability over several months of retests. Downey (**25**) obtained a fair correlation between normal speed of tapping and unspeeded handwriting. Most investigators, however, such as Uhrbrock (**101**), Levitan (**67**), and Reymert (**90**), have used *maximal*[3] tapping speed and have obtained positive correlations with other maximal performances, such as reaction time, writing, etc.

In their own experiment, Allport and Vernon (**2**) utilized (a) finger tapping, (b) hand tapping, (c) leg tapping, and (d) stylus compression, both normal speed and pressure of tapping being obtained in all but (c). The intercorrelations among these and with other motor variables led Allport and Vernon to the following conclusions, which definitely favor a "specificity" interpretation of motor speeds. They state (pp. 177–179):

"Studying all the 14 measures of speed, we find no general speed factor or uniform 'psychic tempo' among our subjects. There is, on the other hand, evidence that each speed measure is itself reliable, indicating a high degree of constancy in 'specific speeds'; in addition, there are three somewhat broad speed factors, namely *verbal, drawing,* and *rhythmic* . . . we find little evi-

[3] The University of London School has employed normal speed of tapping as a measure of motor perseveration (cf. Lankes, **63**, and Bernstein, **8**). Aside from its optimal or maximal use in the attempt to secure an "index of voluntary motor ability," moreover, the tapping test has been employed to obtain an index of right-handedness and to secure an index of fatigue, both preferably in conjunction with other tests of physical capacity (cf. Whipple, **106**, p. 130).

dence for a 'general motility factor' saturating all the measures. The median coefficient for all variables correlated with one another is only .05. . . . Many of the speed measures correlate more highly with non-speed measures than with each other. In short, physical categories of movement are unsuitable models for the *psychological* study of expression."

In a study of consistency in speed on simple mental and motor tests, Adams (1) used as subjects 4 men and 16 women, all Columbia graduates and undergraduates. The tests employed were: (1) normal tapping (tapping at a normal or comfortable rate for 3 ten-second periods with a ten-second rest after each test period), (2) maximal tapping, (3) card sorting (maximal), (4) normal writing (writing "United States of America" 6 times, once on each line at a normal rate; repeated a second time), (5) maximal writing, (6) cancellation (maximal), and (7) arithmetic—addition (maximal). Adams drew the following conclusions:

(1) "The relative daily variability of the 20 subjects for all the tests was low as compared with the average deviation of the subjects from the group. That is, the individual resembled himself more than he resembled other members of the group.

(2) "The subjects were not equally consistent on all tests.

(3) "A subject varied much more from himself in his relative average speed on the 7 tests than he did in his daily fluctuations for each test. Thus no general speed factor was found.

(4) "The tests requiring a normal rate, since they depended on judgment, showed greater inconsistencies than the maximum rate of the corresponding test, which depended at least in part on a physiological limit.

(5) "Tests necessitating attention caused more variation than did those involving only automatic activities.

(6) "The subjects were somewhat less likely to show consistency in the maximum-normal ratios for tapping and writing than in the speed of performance on all tests.

(7) "The average of the correlations in speed between the performance on the first, fourth, seventh, and tenth periods for all tests was high.

(8) "On the whole there was a slightly higher correlation in speed between performances on test periods nearer together than on those farther apart.

(9) "No very clear-cut results were obtained on the subjective reports of the subject as to causes of variation."

Adams thus concludes:

"In general, we can say that as far as these results go, they indicate that an individual subject is relatively consistent in his speed of activity at any particular task at different times. However, a person who is quick in one task is not necessarily quick in another. These results, therefore, do not lend support to the hypothesis of a general speed factor. Subjects differ from each other

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markedly in the consistency of their speed scores, but subjects who are consistent in one test are not necessarily consistent in others. There is no indication that there is a general trait of consistency."

Dowd (23) has also made an attempt to determine the extent to which people who are quick in one task may be expected to be quick in another. The subjects were 165 sixth grade children, whose scores on the Otis Advanced Examination ranged from 35 to 150, with mean of 91.86 and S. D. of 24.19. Several tests were given in which rate of work could be measured, and only those with reliabilities above .90 were used in this study. They were: cancellation (6's according to Woodworth-Wells; underlining a's in Whipple's Manual Number 27010), tests for speed of movement (writing U's; drawing gates), writing tests (writing a sentence; writing numbers), reading tests (passages from a fourth grade reader), and arithmetic tests (Monroe Standardized General Survey). The quality of the subjects' work was approximately constant because of the simple nature of the tests. The responses were counted so that the measure of rate included only examples of the same quality. The intercorrelations between the 9 tests were all positive. Correlations of combined tests were also computed, and tended to give a measure of rate that was more consistent for different performances than were the measures on the separate tests, although some of the coefficients were below .50. Correlations between compounds of tests of different material were computed to see if any arrangement could be found which would conform to the criterion for a hierarchy. On the basis of her series of correlations ranging from .136 to .870, with the preponderance of coefficients at the lower end of the range, Dowd concludes that there is no general speed factor. The only high correlations were between tests with similar content. Thus it was maintained that an individual cannot be rated as "so-and-so" rapid in a general sense; if there is a speed factor it must be of slight extent. Speed as measured by the tests used, furthermore, bore little relation to intelligence test score.

Lanier (62) has also presented experimental findings which indicate that speed is not conditioned uniformly in all abilities by some unitary factor or set of factors. Correlations between speed measurements tended to be high when the postural preparation and the types of motor activity involved were similar. Correlations decreased practically to zero in those cases in which the processes correlated were strikingly unlike with respect to these two major conditions. The results led Lanier to conclude that such expressions as "relation

of speed to mental ability" have no meaning.[4] It is necessary to specify what sort of speed is meant, since different "types" of speed variables have little in common.

Lauer (**66**), in an investigation of personal "tempo" or rhythm, compared typical samplings of voluntary and involuntary response rates. Little relationship was found between specific response rates. Lauer thus concluded that any tendency for bodily tempos to vary together, suggesting a speed factor, would seem to hold only for habitual response if at all. In the latter case, of course, such a similarity would be due to stimulational rather than to biologically determined factors.

Buxton and Humphreys (**17**) have investigated the effect of practice upon intercorrelations of motor skills. They point out that experiments on the interrelations of speed tests in fine motor skills have indicated quite consistently that these tests are either highly specific or at most related only within very narrow groups of movement patterns. Seashore (**95**) has estimated an intercorrelation of +.25 as a representative figure for such tests, but such results have been challenged since learning curves have been involved, and testing may not have been done near enough to hypothetical physiological limits. Buxton and Humphreys maintain that the critical test is to train subjects until they are making little improvement and to compare the intercorrelations between the various motor tests at the beginning and end of the practice period. The subjects employed were 50 male right-handed laboratory students at the University of Oregon, none having had previous practice on the tests. The tests used included the Koerth pursuit rotor, Brown spool packer, and two tapping tests—simple tapping of a telegraph key (used by McCollom, **74**) and the alternate tapping of two brass plates with a stylus, both for speed. A preliminary study determined the respective learning curves and the number of trials necessary to reach approximate limits of improvement on each test.

The results showed that the intercorrelations between the four motor skills at the beginning of practice verified previous findings of low positive relationship between such skills, ranging from +.08 to +.40, approximating a value of +.25. After the practice period, the correlations were somewhat lower, ranging from −.02 to +.39, averaging +.16. Four of the 6 interrelationships were approxi-

[4] For a theoretical discussion of this "fallacy of hypostatization," the reader is referred to J. P. Foley, Jr., The Fallacy of Hypostatization, *J. Gener. Psychol.*, 1937, **16**, 491–493.

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mately zero, suggesting that the effect of intensive practice, if anything, is to make such motor skills more specific. Reliability coefficients for both sets of intercorrelations were high, tending to be slightly higher after practice. Buxton and Humphreys conclude (17, p. 442):

> ". . . that with instrumentally controlled testing and statistically reliable measures, the effect of practice on the intercorrelations of speed in fine motor skill is, if anything, to decrease their relationship, thus upholding previous findings as to the specific nature of the skills tested. Our results do not support the hypothesis of one or a few physiological limits, such as neuro-muscular arm speed, which might be thought of as determinants of success in numerous fine motor speed skills."

These results accord closely with those reported in studies by Walker and Adams (104) and Seashore (96), in both of which a battery of motor tests showed negligible predictive relationship to objectively measured complex practical skills.

In the latter investigation by Seashore (96), for example, 6 tests of the Stanford Motor Skills Unit were administered to 50 operators of high speed winding machines, working under optimal industrial conditions, in the Jantzen Knitting Mills. From an analysis of the data Seashore concludes that " in spite of high reliabilities of both motor tests and the criterion, measures of piece rate under normal working conditions, there was practically no correlation between the separate motor tests or the battery as a whole and the criterion."

Lastly among the statistical studies may be mentioned those which have attempted to investigate the correlation between " speed " and more complex " mental abilities." Kennedy (53) and McFarland (76) have maintained that there is a high correlation between such simple and complex abilities, thereby supporting the " general factor " hypothesis. The bulk of the evidence, however, seems to favor the opposite view.

After a discussion of the literature on the relationship between " speed " and " intelligence " Kennedy (53) reports a study of " irritability." He maintains that there is a tendency for a person who is quick in one task to be quick in another, and advocates the use of the term " irritability " to describe this general rate of work rather than " speed."

In a similar attempt to study the relationship between speed and mental ability, McFarland (76) gave a group of subjects a wide range of tests containing a large number of items. Speed ability was isolated by keeping accuracy constant throughout the series of inves-

tigations. The results led McFarland to conclude that there was a general factor in the various mental tasks presented to the subjects, and that the measures of speed obtained involved general ability similar to that described by Spearman. He maintains that the more intensive and controlled a research of ability in respect to speed, the more positively significant will be the results, certain factors, such as finger dexterity, fatigue, and practice effects,[5] tending to hide the actual relative speed rankings of subjects unless great care is taken in laboratory technique and procedure. These factors, he holds, can be isolated and controlled only by timing each item of a test given to individuals rather than to groups of subjects. McFarland states that there are certain tests where ability in respect to speed does not correlate in a reliable way with quickness in other tests because a " special aptitude " is being measured. Various factors tend to hide the real significance of ability to respond quickly, and thus give rise to poor correlations with rate in the other tests. McFarland concludes that his results agree with those investigators who have held that speed of reaction is one of the most important factors in individual differences in ability to react to mental tasks, and that subjects maintain their relative speed rankings in various kinds of mental reactions. Ability with respect to speed, therefore, is held to be a " general " individual trait which is characteristic of mental behavior. It might be parenthetically noted that neither the reliabilities of the tests nor the size and importance of the " speed factor " are given.

Beck (7) has summarized a portion of the literature on the rôle of speed in intelligence.

He reports that intelligence test scores have been correlated in 14 studies with simple and discriminative reaction, the coefficients ranging from —.32 to +.90; in 14 with serial reaction, range —.03 to +.53; in 5 with speed of reading, range +.14 to +.32; in 6 with speed in serial-verbal tasks, range —.06 to +.23; in 6 with reflex latency, range —.08 to +.24. Whipple (106, p. 144) has reviewed certain of the earlier studies on this general problem, many of which have yielded a positive correlation between rate of tapping and general mental ability.

The bulk of experimental data on the interrelationships among simple and complex speed activities, however, has yielded results in favor of a " specificity " interpretation. Sisk (99), interested in the

[5] It would seem that instead of being considered irrelevant and disturbing, these factors are in reality essential and intrinsic parts of the reaction pattern or experimental situation.

intercorrelations of speed in simple and complex responses and in the relation between speed in different kinds of reaction tests and Army Alpha score, gave 11 different tests of varying degrees of complexity to 100 college students. Approximately 3½ hours was the average time required for completing the 11 tests. From the intercorrelations Sisk concludes that there is no evidence for one who is quick in a simple reaction to be quick in a complex reaction, that there is only a slight tendency for one who is quick or slow in a complex reaction to be relatively quick or slow in another, and that the ability to make a high Alpha score seems to be related to a small extent to ability to react to a complex situation.

In an investigation by Garrison(**42**) to determine the dependence upon the type of performance of the correlation between speed and "higher mental ability," data were secured from 60 students in general psychology. Higher mental ability was measured by the Otis Self-Administering Tests of Mental Ability and a series of 24 analogies. Eight tests were used in which speed was prominent, such as cancellation, tallying, card sorting, vocal responses to different reading materials. Correlations between the speed tests and the higher mental ability scores varied from −.22 to +.34. Intercorrelations between the speed tests showed a higher relationship to exist between those most similar in nature with respect to language or motor type of performance. The performances in which the motor element was prominent gave slightly negative correlations with higher mental ability, never a high positive correlation. In a further study, Garrison (**43**) tabulated the correlations between the scores on a variety of vocal, manual, and printed form tests. These were found to be generally low, except between tests of a common type (as vocal naming with vocal naming, cancellation with substitution, etc.). Thus Garrison concludes that speed would appear to be a function of the ability in a particular task.

The general results of such studies have been summarized by Tinker (**100**, pp. 450–451) as follows:

> "The average intercorrelation between performance in motor tests of various kinds is approximately zero and hence performance is not appreciably affected by any general factor. There is, therefore, no general motor ability comparable to 'general intelligence.' Group factors in motor ability evidently exist, however, for scores in similar kinds of performance such as various types of simple motor reaction yield an average intercorrelation of approximately +.60. Apparently there are many motor capacities or abilities, some of which are specific to a considerable degree.
>
> "Whenever valid measures of intelligence have been employed and when

experimental conditions and size of group have been adequate, no appreciable relationship has been discovered between speed of simple reaction or speed of motor response and intelligence. . . . In view of the results obtained by Farnsworth, Seashore, and Tinker, the correlation of about +.60 obtained by McFarland between simple reaction and various measures of intelligence cannot be accepted as significant until the results have been confirmed."

Perrin (**85**), who found no significant correlations among scores on a wide variety of motor tests (cf. above), likewise concluded (p. 55) that "a test of motor skill cannot be used as a diagnostic measure of character and temperament."

Factor Pattern Analysis

We shall now consider another phase of the statistical approach to the problem of motor speed, viz., the approach through factor analysis. There are many of such studies which have dealt with the general problem of "motor ability" without special reference to speed. The study by Goodenough and Smart (**47**) is of this type. A multiple factor analysis of the scores of 154 children aged 2½, 3½, 4½, and 5½ years on 7 different tests of motor abilities "suggests the existence of at least one common factor running through all the tests and indicates also the probability of one or more group factors entering into several of them."

In a factorial analysis of ability in fundamental motor skills, Jones (**51**) utilized the following measures: chronological age, standing height, weight, vital capacity, running high jump, standing bar vault, rope climb of 20 feet for speed, running 176 yards for speed, throwing baseball for accuracy. The subjects were 2,094 male students. Low intercorrelations between the measures were found. Jones concludes that "the results of the application of the Tetrad Difference Criterion to the data do not indicate the presence of a central factor in the abilities in the nine traits studied." It will be noted, of course, that since not all of the measures utilized were indices of speed, a speed factor is in no way precluded by these results.

Among the studies designed specifically to investigate a general speed factor is that of Bernstein (**8**). He concludes that there is no general speed ability, basing his statement on low correlations, varying from −.228 to +.193, between estimates of "slowness" made on 2 groups of subjects and their "slowness" scores obtained by subtracting scores on "haste" tests from scores on "leisure" tests of sentence completion, direction, concomitants, analogies, and moral

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classifications. The 2 groups of subjects consisted of 70 boys averaging 11 years 9 months in age and 60 boys averaging 13 years. All of Bernstein's tests were short; the leisure tests had fewer items than the haste tests. A constant time allowance of 30 seconds per page was observed. In the leisure tests this gave ample time for all subjects to complete the test, although only the fastest subjects could complete the haste tests in this time. The reliability of all the tests averaged .70, the reliability of the estimates of slowness, .55. Bernstein's methodology has been criticized by DuBois (**27**), chiefly on the grounds that the former's obtained intercorrelations of leisure tests, of haste tests, and the correlations of leisure with haste tests were all about the same, averaging from +.66 to +.69 in one group, and from +.69 to +.73 in the other. DuBois has contended that if leisure and haste tests measure different abilities, their respective intercorrelations should be higher than their correlations with each other.

DuBois (**27**) has sought to discover a common factor of mental speed on a single level of difficulty. One hundred thirty-nine adult subjects of the same sex, race, and educational status were given 5 speed tests, 2 level tests, and 2 tests in which speed and level were not clearly separated. Slight evidence for the existence of a factor common to the speed tests but not affecting the level tests to any great extent was found chiefly in the high average intercorrelations of the speed tests as compared with the low average intercorrelations between speed tests and level tests.

Line and Kaplan (**69**) have investigated the existence, measurement, and significance of a speed factor in the abilities of 64 public school children.

After a preliminary examination of intelligence test scores, tests were constructed which depended on a group factor of speed, and the speed factor itself was finally examined in relation to age, school marks, intelligence, sex, misdemeanors, and perseveration. In addition to the Pintner Group Test of Intelligence, the following tests were employed: substitution, numbers—same and different, easy word analogies, simple addition, words—same and opposite, serial additions. These easy items, when used with a short time limit, gave various measures of "speed." The presence of a speed factor was demonstrated by tetrad analysis. The correlation between speed score and "intelligence" was only +.15±.08. No sex differences were found. There was an increase in speed from trials 1 to 4. Lastly, the authors conclude that the "evidence suggests that speed is subject to improvement with practice, and that this improvement does not run parallel to mental age so much as to speed itself."

Corroborative evidence based on factor analysis for the effect of practice on "general" motor ability, without special regard to speed, has recently been reported by McNemar (77). A study of 5 tests of motor skill, using 9 subjects, gave evidence of a small general factor which became more significant with practice. Anastasi (3) has recently demonstrated experimentally the effect of specific experience upon the results obtained by factor analysis. As stated in the opening of the present review (cf. above), it would seem to the writer that a detailed investigation into the nature and function of the factors operating to condition various motor speeds, thereby producing or failing to produce a "general" speed factor, would be far more important than a mere consideration of the presence or absence of such a general factor itself.

"Character" Type: "Personal Tempo"

Closely related to both the constitutional approach and to the correlational and factor pattern approaches to the problem of motor speed is that which purports to link speed of response to certain "character" or "personality" types. Downey (25) is perhaps best known for this approach, through her emphasis upon what she terms the "Will." In the individual form of her Will-Temperament test, speed of movement is measured by the time required to write the phrase "United States of America," the mean of two trials being used. In the group form of the test, the number of letters written in 20 seconds is employed as a measure. Downey (24) has maintained that graphic speed is symptomatic of general bodily speed of movement, and has reported that it correlates with speed in other diverse activities. She does recognize (25, p. 88), however, "several limitations on the use of handwriting in getting at general speed of movement," and these have been and will be discussed elsewhere in the present review.

Brunner (15) has similarly maintained that a man's output is determined "through his character." Records of rhythmic and arhythmic arm movements are analyzed for the manner in which they were performed, with the view to determining character attributes from the idiosyncrasies in the records. Baxter (5), in a more quantitative investigation, has attempted to classify a group of persons into four temperaments according to the rate at which tasks were performed and the strength (quality or excellence) of the performance. For this purpose he employed a set of four experiments selected to include a great variety of responses, a group of physi-

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ological measures, and the answers to a group of questionnaires about personal habits. The 44 persons studied intensively did not fall into 4 sharply divided groups on the basis of the experimental scores, although there were a few cases consistent as to both rate and strength. The physiological measures likewise gave no evidence of types, and were not consistent with the other experimental results. The rate and strength scores obtained from the questionnaires had the same characteristics as those obtained by means of the experiments. Baxter maintained that there seemed to be indications of a common rate and a common strength factor, but that these factors did not seem to be related to temperament.

These experiments may likewise be criticized on the grounds of inadequate controls. In the absence of an exact definition, the objective status of what these investigators mean by "character," "will," or "temperament" is open to question. In fact it would seem that the initial member of the character-speed correlation is the result of sheer hypostatization.

Wachholder (**103**) has studied the self-chosen rate of movement and its relation to personal rhythm and to economy of movement. The free choice of movement rhythms of the hand and lower arm of 6 subjects was registered when the limits of the movement were set by the experimenter. A relation was found between the rhythm chosen and the amplitude of the movements. Rhythms were most rapid when the amplitude of the movement was small, as compared with those when the amplitude was relatively large. The same subject appeared to choose nearly the same rhythm (variation 20%) on different days for a given amplitude. The rhythms spontaneously chosen were not the same, however, for the hand and lower arm. Individual differences were relatively small for the forearm movement, but more pronounced for movements of the hand. Wachholder concludes that the results obtained do not seem to point to "individuality" as a very important factor in the choice of rhythms, but rather to the existence of mechanical factors, such as elasticity and inertia, which regulate the movements.

Many investigators, such as Wu and Frischeisen-Köhler, have used the term "*personal tempo*" to refer to such a temporal "character" type or "individuality," allegedly representing a personality trait of general speed of response. The "personal tempo" is said to characterize an individual's general motor speed and rhythm, and is attributed to hereditary biological factors. Wu (**109**) made an intensive study of 9 unselected subjects for a single sitting.

In both cases the same 6 tests were used: foot tapping, counting numerals, finger tapping, poetry reading, observing octagon, and word writing. The subjects worked at a "natural" rate of speed. The results seemed to show that in each of the tests, an individual worked at his own characteristic rate or "personal tempo," *i.e.* an individual's relative standing in the group was fairly constant at all times, although Wu admits that the personal tempos were more marked in some tests than in others. In another part of Wu's experiment, the same 26 subjects were used, and the following 6 speed tests were chosen: finger tapping, number naming, foot tapping, packing blocks, word writing, and triangle tapping. The faster one of the records obtained from 2 sittings was used. From an analysis of the results, Wu concluded that although no theoretical "g" factor could be demonstrated, there seemed to be a "general phenomenon" in different tasks. The 2 finger-tapping and the 2 word-writing tests were both positively correlated, with coefficients as large as .51±.10 and .56±.09, while the intercorrelation between all of the 6 tempo tests and all of the 6 speed tests was .19±.13. Thus Wu maintains that for certain tasks which have more or less similar content an individual's natural rate of work or "personal tempo" is somewhat related to his maximal speed.

Perhaps the best known exponent of the concept of "personal tempo" is Frischeisen-Köhler (**38**, **39**, **40**, **41**). In a volume on *The Personal Tempo: A Study in Hereditary Biology* (**38**) and again in a more abridged form in an article on "The Personal Tempo and Its Inheritance" (**39**), Frischeisen-Köhler has defined the "tempo" in various mentalistic and frequently ambiguous terms. Thus she maintains that it "adheres to the individuality as a whole," and contends that as an integral unity the "psyche" abhors one tempo as "unsympathetic" and recognizes another as "sympathetic." She holds that one must work with simple characteristics and functions, rather than character-complexes, in order to discover what she terms the "biological laws of inheritance."

In order to ascertain the "hereditary" component of the personal tempo, Frischeisen-Köhler (**38**, **39**) conducted tapping and metronome experiments. The former consisted in: (a) tapping against or upon the table with a spontaneously chosen position of the hands or fingers; (b) tapping on the floor with the foot; and (c) tapping the edge of the table with the outstretched forefinger, for 10 seconds in each case, with the instructions for the subject to "tap in the tempo most agreeable to him or her." In the latter or metronome experiments, the subject was given several tempi, one after the other, and was to say, in the case of each tempo, whether it was too slow, too quick, or precisely agreeable. The subjects tested included 1,000 individuals of both sexes, all ages, and "varied environments." The results indicated a seemingly high degree of intra-individual con-

stancy, although limited statistical analysis is reported. This constancy was greater in the metronome than in the tapping experiments. Marked inter-individual differences in tempo were noted, although the personal tempo seemed to be altered very little by certain environmental differences. Frischeisen-Köhler concluded (**39**, p. 305):

"The social milieu [6] had no influence on the tempi registered; the extreme tempi, on the slow side and on the fast, the distribution of the various classes of variants, and the mean value of the tempi, were the same in the different social strata. . . . The inter-individual index of variability was likewise the same in both social groups."

A further study of the personal tempi of twins (118 pairs), siblings, and unrelated individuals led Frischeisen-Köhler to conclude that the personal tempo is definitely conditioned by heredity. From a comparison of the percentage deviations of monozygotic and dizygotic twins (of the same sex), she estimates (**39**, p. 308) that "the inherited disposition counts for about 50%" and states that "this figure, which is to be regarded as the minimum share of hereditary determination, has already been obtained in respect to physical and other psychic characters." It was found, for example, that the percentage deviations of unrelated individuals was 2½ times as great as those of monozygotic twins, Frischeisen-Köhler concluding (**39**, p. 309) that "we must refer these great differences to the very much greater differences in inheritance in unrelated individuals." The fact that the environmental conditions are undoubtedly more similar in the case of twins and that this factor alone might readily explain her results is completely ignored.

Lastly, Frischeisen-Köhler investigates what she terms the "mode of inheritance" of the personal tempi. A comparison of the tempo of 85 pairs of parents and 318 children revealed a striking agreement between the tempi of parents and children, the number of children in the quick tempo group decreasing in proportion to the slowness of one or both of the parents. Frischeisen-Köhler at times appears to have recognized the environmental implications of these latter results, but she quickly resorts to her hereditarian hypothesis. She states (**39**, p. 310):

"If we had only the results of our experiments with families to go upon, we might conclude that the education and environment of the children alone evoke a tempo similar to that of the parents.[7] By our investigation of twins,

[6] The social milieu was taken into account only in the case of schoolchildren from the upper and public schools.

[7] No cognizance is taken of the fact that the parents and children are part of each other's environments.

however, we have established the hereditary determination of the personal tempo, and we are now justified in saying that the inherited factors, whose combination with the existing environmental stimuli affecting one or both parents results in a definite personal tempo on one side or the other of the mean, work in the same direction in the children."

She sets forth an hypothesis of inheritance dependent upon several "homomeric allelomorphic series," and concludes (**39**, p. 313) that the results "have shown that it is possible, by the principles and the methods of scientific genetics, not merely to demonstrate the inheritability of psychic characters, but also to form hypotheses as to the mode of their inheritance."

In 2 other studies, Frischeisen-Köhler (**40**, **41**) has used the metronome to investigate the sensitivity to speed differences and the problem of optimal speed or tempo.

In the former study (**40**) the subjects were 216 boys and 215 girls, aged 8–16. Among the boys 39.8% gave correct judgments, as compared with 36.3% of the girls. The age levels 4–12 years showed an increase in accuracy of judgment. In the latter study of neutral or most agreeable speed (**41**), a speed of 72–108 strokes per minute was judged medium by 67.93% of the males and one of 76–108 strokes by 59.67% of the females, showing that there is a neutral range for a large number of individuals, although the entire range of the metronome was judged medium by others. The preferences followed the normal frequency distribution, the curve of the females lying toward the faster side. It was found that after a series of slow speeds had been given, the subject would occasionally judge this speed as medium. By studying twins, Frischeisen-Köhler concluded, as in the experiments reported above, that the personal tempo is largely hereditary.

Social and Occupational Factors

We now come to what is perhaps the most important of all the factors having a possible influence on motor speed and tempo: viz., practice or experience. We shall consider only the most representative of the many different approaches which have been made to this particular problem, one of the most important of which involves an analysis of occupational or professional differences in speed, tempo, and rhythm of motor behavior.

It was previously pointed out that Downey (**25**), notwithstanding her hereditary bias, listed "practice" as one of the "limiting" factors which affect the speed of handwriting, which she considered diagnostic of general bodily speed of movement. She states (p. 88):

"It [practice] may even be cited by critics as the most influential factor in determining speed of handwriting. . . . Practice is not, however, the sole determinant of speed, as is shown by the fact that equally practiced penmen

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write at very different rates. The different systems of writing used may account in part for this difference in speed, but even with the same system of penmanship, writers vary in speed in obedience to such general factors as 'drive,' forcefulness, and carefulness." [8]

Nor did Downey fail to recognize that the factor of practice might reasonably be expected to be reflected in social and professional differences in speed and tempo, for she contends (pp. 88–89):

"When the amount of practice varies widely as between individuals of differing social status or of different professional requirements, practice should be taken into consideration. Different norms should be established for different social and professional classes . . . different norms for different social groups."

The important rôle of socio-economic factors in simple motor response is indicated by an early study by Bolton (11), published in 1903, and reported as an investigation of "the relation of motor power to intelligence." Two classes of children were employed as subjects:

(a) "those from the best wards of the city (Lincoln, Nebraska), where the general home surroundings were the most wholesome and cultivated.

(b) "others from the lower wards, where the largest percentage of poor foreigners is to be found, and where the hygiene conditions of the home as regards food, clothing, sleeping, air, and light are bad."

The author states (p. 353) that "the best people are good representatives of the best class of Americans, and the poor people are a mixture of various nationalities of foreigners, mostly Russian Germans." The experimental results indicated that the children drawn from the better social classes were uniformly superior to children of the poorer class, both with the right and with the left hand. The fact that the divergence was greater at 9 than at 8 years of age was attributed to a general arrest of development in the poor-class children. The "good" children showed a distinctly greater practice improvement, which Bolton thought indicative of a fundamental difference in the ability of the two classes of children to adopt new habits and profit quickly by experience. It is interesting to note that although the title of Bolton's article, together with his interpretation of the results, would indicate that the study dealt with ("native"?) intelligence as a factor in motor ability, the study actually yields data on the problem of socio-economic factors in motor performance. Bolton's biased interpretation, even in the face

[8] It evidently did not occur to Downey that such factors as these might also be of experiential origin.

of data to the contrary, is typical of the mentalistic views of many of the earlier workers, many of which views are still prevalent today.

It will be recalled that the fourth of Seashore's hypotheses (95) concerning the determination of individual differences in motor performance was that of practice or experience. Seashore describes this hypothesis (p. 49) as "the ascription of individual differences in various motor tests to differences in previous training, both direct and transferred, and in all degrees of efficacy," and adds that this hypothesis is "perhaps the most difficult to evaluate." His own study touched upon this hypothesis.

Fifty men were tested on 8 serial motor performances, knowledge of results being available throughout the experiments. Reliabilities of the tests averaged .84, and intercorrelations of tests ranged from —.15 to +.63, averaging +.25. Highly selected athletes excelled on 2 pursuit tests and were average or slightly above average on the other tests. A few pianists excelled in the serial discrimination and motor rhythm and were approximately average on the remaining tests. Ratings on amounts of previous training in athletics, instrumental music, typing, and other motor skills tended to show that these types of training did not have very great carry-over. Intercorrelations of the separate motor tests with the Thorndike College Entrance Examination ranged from —.33 to +.23. Correlation of the Thorndike Examination with the battery of equally weighted motor tests gave a coefficient of —.14. Seashore concludes (p. 62) that "the independence of skills measured in these tests argues against any theory of general motor ability and in favor of specific skills. . . . The theory by which motor skills are determined by a relatively small number of basic motor capacities is strongly open to question."

Shaffer (97) has likewise used athletes in an investigation whose purpose was "to compare different groups in certain accepted tests of motor ability with reference to their ability to make small muscle movements and to compare these same groups in a mental factor, which for the want of a better term, we may call alertness." Three groups were used: team athletes, individual athletes, and non-participants in any form of athletics. The following tests were employed: steadiness, tapping, strength of grip, endurance, cancellation, an omitted letter test, a tachistoscopic test, and a general intelligence test. The general conclusions drawn by Shaffer are: (1) athletes are superior to non-athletes in motor ability and alertness; (2) there is a difference in motor ability and alertness within the athletic groups, and this difference appears to be in favor of those athletes who participate in team games in contradistinction to those who take part in individual athletic feats; and (3) athletes are inferior to non-athletes in tests of arithmetic ability, precision in language, and general information. It must be kept in mind, of course, that

Shaffer's results merely indicate certain relationships between athletic ability and particular motor traits, and are not conclusive with respect to the factors contributing to such relationships. Selective factors, for example, have not been controlled.

An early study on musicians by Binet and Courtier (9) has certain psychological as well as pedagogical and artistic interests. By means of recording apparatus and tambours, placed under the piano keys, objective indices were obtained with respect to a number of factors, such as pressure exerted, time, form, accentuation, held notes, fingering, intervals, and speed. The latter problem of speed included speed of attacking a note, the maximal number of notes played in a unit time, and other modifications resulting from increased speed. Binet and Courtier conclude that the maximal number of notes played cannot be dogmatically stated, since this depends on a number of factors, such as fingers, their simultaneous or successive play, and so forth. The amplitude of the notes is diminished with increasing speed. They conclude (p. 222):

". . . il n'existe point de *mécanisme impeccable;* on n'atteint jamais la perfection, la regularité absolue, mais on s'en approch plus ou moins; en d'autres termes, l'incoordination se montre dans le jeu rapide, et elle requiert, pour se montrer, un jeu d'autant plus rapide que l'exécutant est plus habile."

Raif (89), in an investigation on 18 music pupils, has obtained results which are in general similar to those of Binet and Courtier. Whipple (106, p. 136) summarizes the results of the 2 studies as follows:

"The effect of piano practice, as the investigations of Binet and Courtier and of Raif show, is to improve coördination of movement, *i.e.* its regularity, smoothness, etc., but not to increase the natural capacity for speed or rate of movement."

This conclusion does not seem justified, however, since Binet and Courtier and Raif report no data which bear directly upon this problem. Their results do show a definite practice effect with respect to coördination of movement, however. Binet and Courtier, for example, report that Mlle. Blanchard, a distinguished pianist, showed regular movements in a trill, whereas an individual who does not know how to play the piano shows marked incoördination after the first 10 notes of the trill.

A number of studies have investigated the effect of practice in a given vocation upon some aspect of manual ability. Thus Ehinger (28) attempted to see whether the practice of a manual

trade would influence the results obtained from certain tests of manual ability. Four tests were given to 25 apprentices and to 25 journeymen who had just entered a factory. When the same tests were given again after an interval of several months, marked improvement was noted, whence Ehinger concludes that the exercise of a trade has an effect in training general manual aptitude. The need for a control group which would repeat the tests after the same time interval, although without the intervening trade experience, is of course obvious. As would be expected, the improvement was much greater in the case of the apprentices than the journeymen. When in each test the correlation between the first and the last trial was computed, and when the correlations so obtained were compared with the correlations between the mean of the first trials for the four tests and the mean of the second trials for the same tests, it was seen that these last correlations were much higher, this fact being interpreted by Ehinger to be evidence for a general manual ability. It was found, moreover, that subjects with a higher initial ability, measured by the tests, were not surpassed after a certain time of apprenticeship by other individuals whose initial ability was less than theirs. There was also a general tendency for individuals with poor initial ability to make better progress with practice than those with better initial status.

Engel (**29**) has also made an analysis of the influence of practice on the speed of work. On the basis of learning curves in industrial operations, Engel has pointed out the error in comparing curves where individuals have unequal amounts of previous practice, and has compared curves to show that operations with longer unit-times show greater relative practice effects. In a similar study on the influence of practice on working speed, Meyer (**78**) obtained negatively accelerated learning curves in 3 factory operations where workers were timed over a period of 39 days.

The voluntary motor ability of the world's champion typists has been studied by Book (**12**), who gave the Bryan motor control tests (cf. **16**) to all contestants in the International Typewriting Contest held in New York City in October, 1923. Book's interest was primarily practical, concerning the possibility of devising "tests for selecting typists and learners of typewriting" and "to determine what some of these necessary abilities for learning to typewrite are." The Bryan tests involve measurements of the rate of movement of finger, wrist, forearm, and upper arm; records were taken for both left and right members respectively. The subjects tested (48 cases

in all) were the winners in more than 80 state and district contests throughout the world, and were divided into 5 classes: professionals, amateurs, novices, world's school novices, world's school class. Complete records were also obtained on the motor ability tests from 5 ex-world champions and from one professional who gave public demonstrations in speed typing throughout the country. In addition, Book tested 65 students in typewriting at Indiana University, and compared their records with the available Bryan norms.

The following were among the conclusions drawn by Book (12, pp. 305–306) following a comprehensive analysis of the data:

1. "That the world's champion typists possess an exceptionally high degree of voluntary motor control and that this superiority holds for each of the 8 sets of voluntary muscles tested.

2. "That this same superiority in voluntary motor ability is found among the ex-world champion typists and was present among all the contestants in the last International Contest in almost the same proportion to the skill in typing which each displayed.

3. "That there is a greater consistency in voluntary motor control among the members of these various groups than is found among unselected individuals of corresponding ages, suggesting that all individuals who do not possess such high degrees of voluntary control were eliminated in previous contests.

4. "That the increased voluntary motor control possessed by the expert typists was not developed by practice in typing because the group of beginners studying typewriting . . . possess as much voluntary motor control as the experienced typists in the same school. . . .

5. "That the present and ex-world champion typists possess an unusual amount of voluntary motor control in the forearm, shoulders, and left hand and arm."

It is interesting to note, in connection with Book's last conclusion, that the expert typists showed their greatest superiority over their corresponding age norm in the elbow and shoulder movements, and were particularly superior in movements of the left hand. It is also significant that the mean variation for the experts was less for the left hand than for the right. The data obtained on the beginning and advanced students at Indiana University (cf. conclusion 4 above) are inconsistent with the major results on the rest of the experiment. Book states the practical conclusion (p. 307) that "a superior degree of voluntary motor control is the only necessary characteristic that a superior typist must possess," although his data certainly do not answer the basic problem as to whether this superior motor control found in the expert typist is the result of biological or selective factors or is the result of the practice effect from the typing itself.

Link (70) has also investigated certain possible uses of tests of

simple motor ability as an index of vocational aptitude, in a study of the relationship between tapping and efficiency in factory work as measured by actual production. Link's subjects were 73 girls in a firearms factory. In a group of 52 girls employed to inspect shells, the correlation between production and speed of tapping was only +.14; but in a group of 21 girls engaged in fitting shells into a gauge, the correlation was +.52. Link has pointed out that the job of gauging shells is largely motor, while inspecting is mainly visual, and has suggested that this difference probably accounts for the rise in correlation. As has been pointed out by numerous investigators, when we remember that correlations between mental tests and school success are seldom higher than .50, Link's result for gaugers is rather striking.

Kitson (54) studied typing and piano playing, both of which apparently have a direct relationship to tapping activity, in an attempt to determine whether the tapping test measures aptitude in these two vocations. The subjects were 25 women pianists who had studied from 4 to 11 years, and 25 women typists. These 2 groups were matched against control groups of the same sex and ages but untrained in the given vocations. Only slight differences were found between the vocationally selected groups and those unselected. From his own work and that of other investigators, Kitson suggests that the tapping test may differentiate between ordinary typists and typists of extremely great ability, but that this relationship is unimportant for vocational guidance purposes. He found, however, that some of the separate movements of the typists and pianists were superior to those of the unselected control group, even though the total of all the movements was not. The reason for this, according to Kitson (pp. 196–197), is as follows:

"While one of the vocational groups may have excelled in tapping with one part of the body, they may have been inferior in tapping with another part, and thus the net difference was reduced. For example, although the pianists surpassed unselected women in tapping with three of the members of the left side (wrist, forearm, under arm) they were inferior to the latter in tapping with the finger on both right and left hands. A ready explanation of this can be offered. In both piano playing and typing the finger is kept curved, the end of the finger striking the key vertically. In the tapping test, however, the stroke had to be made with the finger held in a horizontal position. This is markedly opposed to the habits formed by pianists and typists and it undoubtedly caused much interference. A number of the subjects mentioned the fact and they had to be constantly cautioned by the experimenter."

Foley (37) has recently investigated the cultural conditioning of preferential and maximal motor response and of sensory (auditory) preference. More specifically, the problem was to investigate the speed of preferential and maximal tapping as well as that of the preferential metronome rate in various occupational (cultural) and "racial" (biological) groups, with the view to determining the underlying factors responsible for the particular behavior manifested.

The subjects were 684 female students, aged 13–20, at the Yorkville Vocational High School, in New York City. The following occupations were studied: power machine operating (N=90), trade dressmaking (N=180), beauty culture and personal hygiene (N=165), commercial typewriting (N=182), and domestic occupations (N=67), including cafeteria, tea-room management, and waitress training, as well as home nursing, hygiene, first aid, and nursery education. The vocational groups were comparable in chronological, "racial" or national, socio-economic, and general intellectual status. The following indices were experimentally obtained or computed for each individual subject: optimal (convenient), maximal, and ratio of optimal to maximal speed of tapping (telegraph key technique employed) for the index finger of the preferred and non-preferred hands; the ratio of the preferred to non-preferred hands for the optimal, maximal, and ratio of optimal to maximal rates of speed; as well as the preferential sensory speed judged (according to a modified psychophysical method of limits) from a series of metronome rates.

In brief, the results showed that the dressmaking (hand sewing) and typewriting groups were fastest with respect to optimal or most convenient tapping rate for both preferred and non-preferred hands, with the beauty culture group intermediate, and the power machine operating and domestic occupation groups definitely slowest. The same results were obtained for maximal speed of tapping with the single exception that the beauty culture students resembled the dressmaking and typewriting groups in making high maximal scores for both hands. The ratios of optimal to maximal tapping rates for each hand corroborated the above findings. The differences cited were statistically reliable. Foley has concluded that the results conform to what would be predicted on the basis of an empirical theory of the cultural conditioning of motor traits, since the characteristic and habitual trade movements of the occupations employed range from the rapid, precise, discriminative, and repetitive movements involved in dressmaking and typewriting, through those in beauty culture, to those in power machine operating and the especially slow, variable, and relatively gross movements of the domestic occupational group. In the metronome experiment, the fastest preferred rates were evidenced by the typewriting and power machine operating groups,

whose mean scores were markedly (statistically reliably) higher than those of the girls engaged in beauty culture, dressmaking, and the domestic occupations. These results were again what would be expected on the basis of environmental factors, since the loud and rapid repetitive noises from the typewriters and power machines offer a constant source of auditory stimulation for these groups of subjects, whereas such is not the case in any of the other vocations studied. Trends within each of the groups studied, obtained by comparing the tapping and metronome indices obtained from beginning and advanced sub-groups, confirmed this hypothesis, the more rapid groups on a given function speeding up and the less rapid groups slowing down, on the whole, with increased time spent in vocational training and experience. Lastly, when the subjects were divided into " races " or nationalities, with the following groups large enough to yield data worthy of statistical treatment: Italian (N=305), Negro (N=86), Jewish (N=53), Irish (N=55), and German (N=55), the differences in the above-enumerated tapping and metronome scores were small, inconsistent, and neither statistically reliable nor logically significant. Thus, contrary to the conclusions drawn by Frischeisen-Köhler and others, vocational stimulation and associated institutionalized responses seemed to play a major rôle in conditioning the speed of preferential and maximal motor response and of sensory (auditory) preference.

The above studies on the effect of practice or experience have been chiefly concerned with the phenomenon of motor *speed*. There are a number of similar investigations which have dealt with practice effects on motor *rhythm*, and it is to this last topic that we now turn our attention. As in the case of motor speed, subjects with specific occupational experience have frequently been utilized to investigate possible practice effects.

In a study of motor rhythm, Seashore (**94**) found that neither speed of tapping nor reaction time were significant for precision of motor rhythm. He reported (p. 188) that " a group of superior amateur musicians showed a high average rank, but scores on the test are found to be very slightly affected by training in music or continued practice on the test itself." In a later study of individual differences in motor skills (cf. above), however, Seashore (**95**) found that pianists excelled in serial discrimination and motor rhythm, although they were approximately average on the remaining tests.

Jersild and Bienstock (**50**), in a study on the development of rhythm in young children reported above, obtained cinematic records

on 94 children aged 2–5 years and on 17 adults, as they walked or beat time with their hands to the accompaniment of piano music. Analysis of the results indicated that the average scores rose substantially from 2 to 5 years, the adults being about twice as proficient as 5-year-olds. The authors concluded that "practice," in the form of 40–50 opportunities to keep time to the music, brought an increase in score, but not as large an increase as obtained in the case of the so-called "unpracticed" older children.

Koch (**59, 60**) has conducted a number of studies on the work rhythms of agricultural workers in Germany.

One such study (**60**) involved an analysis of the work rhythms of both men and women workers engaged in sowing fertilizer, loading wheat, threshing, cutting the leafy tops from sugar beets, mowing grass, and digging potatoes. Individual work rhythms were found which differed from worker to worker, but seemed to remain constant from one work period to another and from task to task. Another representative study (**59**) comprised an analysis of the effects of different types of hay wagons on the work rhythm curve, as well as additional experiments on threshing and cutting tops from sugar beets. The results substantiated the author's former findings that different types of work produce different work rhythms, and that individual differences in rhythms exist, and, in a general way, remain constant when once formed in spite of all the variations arising from extraneous influences.

A number of experiments have clearly indicated that motor rhythm is subject to specific practice effects.

Lau (**65**), for example, in an investigation on the modifiability of personal rhythm, found that the efforts of the same individual to beat time during various activities did not reveal an identical rhythm. Nielson (**81**) tested 20 students chosen at random and 20 Music School students on the Seashore phonographic chronograph. Ten of the latter group of students were ranked as being high and 10 as being low in rhythmic playing as determined by instrumental work. It was found that capacity for motor rhythm correlated significantly with musical ability and performance, and that motor rhythm could be slightly improved.

Farnsworth and Poynter (**33**) have described a case of unusual ability in simultaneous tapping in two different times.

In brief, the report deals with a test of the ability of Arthur Hardcastle, a modernistic composer and performer well versed in the cross-rhythms of ultra-modern music, to tap complicated rhythms, where the tapping with the left hand is considerably different from that being done simultaneously with the right. A modified Weiss-Renshaw polygraph was employed for recording the taps, the subject tapping on telegraph keys, one in each hand. Rhythms of 2:3, 3:4, 3:5, 3:7, 4:5, 4:7 were studied, the first number of each pair representing the number of beats tapped by the left hand, and the second the

number tapped by the right. In every instance the left hand took the slower tapping rate. In general the results were very good. In no case was there an error of over 1/10 the distance from one tap to the next. Although norms are not presented for a general unselected population, it is certain that such a performance is well beyond the ability of the average individual.

Additional experiments on the simultaneous tapping of different rhythms with the 2 hands have been performed by Ricci (**91, 92**).

In one such experiment (**91**), the subjects were instructed to react with two movements in accordance with the beating of two metronomes. Subjects chosen at random could not execute the 2 movements simultaneously with the metronomes. Pianists could make one movement in accord with the given rhythm and the other in accord with a spontaneous rhythm, or both movements in accord with spontaneous rhythms. Only one orchestra director was at times able to follow the 2 metronomes. In another such investigation, Ricci (**92**) instructed his subjects to tap 2 different rhythms with both hands simultaneously. Unpracticed subjects were unable to do this, but departed with both hands from the prescribed rhythm, although they tapped definite rhythms. Telegraph operators and some pianists did likewise. Some pianists were able to follow the rhythm for one hand, but the other moved in a spontaneous tempo. These results, together with those of Farnsworth and Poynter, indicate the marked effects of practice or occupational experience upon this particular motor ability.

An example of the effect of practice upon an entirely different type of sensory-motor performance is afforded by the study of Flory (**34**) of the effect of art training on mirror drawing. Students trained in art were found to be nearly twice as efficient in mirror drawing at the start of the experiment as an untrained group. Both groups increased rapidly in efficiency during the early trials. There were some indications that art students were bored more than untrained subjects after the artistic possibilities of the design had been exhausted. Untrained subjects could reach the efficiency of the trained group if sufficient time were allowed and sufficient energy expended.

Summary

A consideration of the material here presented will indicate quite clearly that the bulk of the experimental evidence supports the contention that speed of reaction is conditioned by specific environmental factors, such as the stimulation arising from prolonged occupational experience. The data were either clearly negative or the results inconsistent with respect to the operation of physiological factors, age or maturational factors, sex differences, "constitutional" type, and "racial" type. Although not in unanimous accord, most of the

data derived from intercorrelational and factor pattern analyses have favored a specificity interpretation rather than one based on a "general speed factor." The sporadic results presented in support of an hypostatized concept of temporal "character" type or "personal tempo" have not only been obtained under inadequately controlled conditions, but have been refuted in large measure by verified experimental results. In marked contrast to the above, vocational stimulation and institutionalized motor responses occurring at a particular rate of speed were found to play a major rôle in conditioning the speed of motor response (optimal and maximal) and sensory (auditory) preference, as well as of motor tempo and rhythm.

A number of psychologists have emphasized the environmental conditioning of motor speed. In his work on "Les techniques des corps," Mauss (**73**) has stressed the social nature of such habits. He points out that habits vary not simply with individuals, but especially with the society and its fashions, education, and objects of prestige. He maintains that it is not sufficient to know the physiology of a given movement, but that one must also know the traditions of the society in which it takes place. Kantor (**52**), perhaps more than any other psychologist, has stressed the social or institutional nature of most motor and other behavior equipment and the inviolate dependence of such behavior upon the specific reactional biography of the individual. Sherif (**98**) has recently made a similar point, phrasing the problem in terms of social frames of reference which are empirically determined.

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BOOK REVIEWS

KANTOR, J. R., *An Objective Psychology of Grammar.* Indiana University Publications Science Series No. 1. Bloomington, Indiana: Indiana University, 1936. Pp. xvi+344.

The psychology of language is a neglected field of study. Psychologists have given it but little attention, and that little has not been very helpful to linguists. Linguists have tried their hands at describing the psychological nature of language, but the outcome has not, on the whole, been very satisfactory to psychologists. Consequently, where the two disciplines should overlap, there remains a very definite gap in the literature. In *An Objective Psychology of Grammar,* Kantor has tried to close that gap by applying systematic psychological concepts to the problems of formal grammar.

The monograph is divided into three parts and 22 chapters. Part I outlines the general program of the work, discusses traditional attitudes toward language and grammar, and gives an elementary summary of the psychology (organismic) to be used in the developments of Parts II and III.

From the standpoint of psychology, the fundamental objection to the grammarians' treatment of language is that it deals with dead things, symbols on paper, with forms, and artificial classifications instead of with living language. Such formalism persists even though its sterility has frequently been condemned. Gorki, for example, wrote: "Especially was I oppressed by the grammar with its monstrously narrow, stiff forms. I was altogether unable to squeeze into them the living, difficult, and capriciously flexible Russian language" (quoted by Kantor, p. 17, note). A real psychology of language, one which would help both the linguist and the psychologist to understand an actual linguistic event, must go directly to everyday living language for its raw data. A number of grammarians accept this point of view in the abstract, but in actual contact with linguistic problems, Kantor charges, they frequently fail to live up to it. Their recognition of the behavioral aspect of language has not entirely freed them from the formalism of their grammatical background and they have not gone far enough to satisfy Kantor.

The most fundamental and in some ways one of the most puzzling of linguistic problems is the question of just what constitutes lan-

guage. Kantor answers that language is a response to two stimuli. In using language one refers to something (the adjustmental stimulus) with a reaction (usually, but not necessarily, verbal) which is directed toward another person (the auxiliary stimulus). For example, in the utterance *Open the window,* the speaker responds vocally to the adjustmental stimulus (the window) and directs his response to the auxiliary stimulus (the hearer). Both stimuli sometimes coincide, as when one says *You look well today.* The speaker is here simultaneously reacting to you (the auxiliary stimulus) and referring to you (the adjustmental stimulus). The form of the response is immaterial for Kantor. If the host asks *Have another?,* the guest's response may be *I believe I will, Yes,* a nod, or any other appropriate gesture. These would all be classed as language since all are reactions made to the host (the auxiliary stimulus) and adjustments to whatever he is offering (the adjustmental stimulus).

There is much to be commended in this definition. It emphasizes the way in which language is actually used—as a means of adjusting to a situation. It avoids the pitfalls of definitions phrased in terms of symbols—Kantor's pet aversion in linguistics. And it leaves unspecified the nature of the adjustmental response. Vocal speech, gestures, or writing responses may all meet the requirements of the definition and be classed as language.

To call more emphatic attention to what language *is* Kantor points out three kinds of things which true language is not. The three outcasts are dead language, nascent language, and spurious language, all three of which differ from living language in that they lack the adjustmental character which is the essence of true language. Dead language includes things which may once have been living but are now dead, such as words on paper or in one's memory. Nascent language includes things which may at some future time become living but are not now, such as Esperanto or a revived Hebraic language. Spurious language includes characters, signs and symbols, literature, verbal formulæ, verbal records, and liturgical speech. For the sake of contrast, some examples of genuine language, listed in decreasing order of aliveness, are: conversation, oration, conventional speech, prayer, letter writing, and pamphleteering.

While the distinction between genuine and non-genuine language can be made quite easily in a formal fashion, a number of questions arise when one attempts to illustrate the difference. "*How do you do?, Good morning, Greetings,* and *Fine day*" (p. 21) are spurious language because "They . . . have never constituted nor are likely

to constitute referential adaptations to speaker and thing spoken of" (p. 21). However, "When we call upon a friend . . . The adjustment consists in the friendly greeting and conversation" (p. 35). The friendly conversation is the best possible example of language as Kantor defines it, but the greeting is ambiguous. Standardized greeting formulæ are classed as spurious language while a friendly greeting is accepted as an adjustment and hence as true language. When the standardized *Good morning* is said in a friendly manner, is the language function to be found entirely in the facial appearance, the tone of voice, and the manner which accompany the formalized greeting? As a second problem, are *The Letters of William James* letters (living language) or literature (spurious language)? Again, in the sentence used by an Oriental merchant, "As God is my witness I cannot take less" (p. 83) Kantor says that *I cannot take less* is true language because it embodies an adjustmental reaction, but *As God is my witness* is spurious language since it is not an adjustment but only an attempt to symbolize the merchant's sincerity and veracity.

These appear to be rather finely-drawn distinctions, but if one accepts the basic definition, non-adjustmental speech cannot be classed as language. Difficulty arises, however, in trying to decide just which speech reactions are and which are not adjustmental in character. Kantor answers that in actual speech, where one could hear the whole conversation and see the total situation, such problems would disappear.

In Part II of the monograph Kantor discusses Semantics, Syntax, Morphology, and Phonology, and in Part III Speech Parts, Person, Gender, Case, Tense, Number, Voice, Mood, Direct and Indirect Speech, and Negation. A chapter is devoted to each topic. In each chapter he follows a general plan of summarizing the traditional views, suggesting some of the difficulties inherent in them, and then outlining the way in which his own psychology and definition of language would handle the problems. One or two examples will illustrate his manner of treatment and thinking.

Traditionally speech utterances have been classified into three persons, first, second, and third. Some utterances fall easily into one of these three categories, but difficulty is encountered with others. The confusion lies in the fact that *person* is used ambiguously. Does the person of a statement refer to the speaker or to the form of word used? When John Brown writes *John Brown accepts with pleasure,* is he using the first or the third person? When one uses the editorial

We, is he using the first person singular or the first person plural? When one asks *Who is there?* is he using the second or the third person? In trying to answer such questions, grammarians have usually fallen back onto the form of the expression as a criterion, a method which seems very obviously wrong to Kantor, who stoutly maintains that the nature of the adjustment involved should always be the criterion invoked.

The problem of person, Kantor claims, cannot easily be handled on the basis of the tripartite nature of language. There are as many persons as there are possible organizations of the speaker's reference to the adjustmental and auxiliary stimuli. Seven such organizations, or persons, are defined and illustrated. They are: (1) the speaker is also the adjustmental stimulus, *e.g. I am ill*; (2) the person spoken to is also the adjustmental stimulus, *e.g. You did that yourself*; (3) neither the speaker nor the hearer is the adjustmental stimulus, *e.g. It is raining* or *He came yesterday*; (4) the adjustmental stimulus includes both speaker and hearer, *e.g. You and I are going*; (5) the adjustmental stimulus includes the speaker and a third individual, *e.g. John and I are going* or *I stepped up to the man*; (6) the adjustmental stimulus includes the hearer and a third individual, *e.g. You and John are going;* and (7) the adjustmental stimulus includes speaker, hearer, and a third individual, *e.g. You and John and I are going* or *All of us will go.* The English language has specific words for only the first three of these seven persons and must use combinations of terms to express the other four. Such a language as Samoan has specific words for all seven.

In several chapters, Person, Tense, and Voice, for example, Kantor criticizes severely a common grammatical practice of interpreting different forms of expression as implying different forms of ideas and as meaning that the number of tenses or persons varies from one language to another. Differences in verbal forms are due, Kantor believes, solely to differences in custom and training and imply nothing with regard to the nature of the psychological adjustment made. Thus, it makes no difference whether we express the fourth of Kantor's seven persons with such an expression as *You and I* (the English method) or with a single word (the Samoan method). It makes no difference whether we express future activity by a special verb form as is done in many languages, or by prefixing *shall* or *will* to the infinitive as is done in English. In general, there are just as many tenses or persons in one language as there are in any other. The differences in verbal forms illustrate interesting dif-

ferences in custom and training but are irrelevant from the standpoint of the underlying adjustment, the psychologically important factor.

Kantor's purpose in writing the present monograph was dual: negatively, to point out some of the most common misinterpretations of the psychological nature of language; and, positively, to offer a systematic application of his own system of psychology and definition of language to grammatical problems. In discussing the inadequacies of the traditional grammarians' psychological concepts, Kantor gave himself a task which might all too easily have led to the old trick of setting up a straw man to destroy. But there is more substance than straw to what Kantor has attacked, and the attack has been a fair one. While objections from some grammarians are almost inevitable, others have already expressed high praise of the work. Just how satisfactory the positive aspects of the work will be to an individual reader will depend, in part at least, upon whether or not he accepts Kantor's psychology and definition of language. But whether they are accepted or not, the monograph provides an excellent basis for consideration of a large number of problems in the psychology of language. Technically too, the work is quite satisfactory. It covers all of the main topics of grammar. There are subject and author indexes and a classified bibliography. The work is largely free from typographical errors and such errors as occur are minor.

Unfortunately, there are some less favorable aspects of the book. One is serious. A number of passages are marred by a lack of clear writing that makes it difficult to understand the author's meaning. Occasional statements are unnecessarily puzzling because they are not cleared up until some pages later. In a few cases there even appear to be definite contradictions between ideas expressed in different parts of the book. But these defects are less important than the fact that the various grammatical categories have all been discussed in terms of one unified psychological system. That is an achievement of which Kantor may well be proud, and one with which others whose interests lie in the field of language should be thoroughly familiar.

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JANET, P., *L'intelligence avant le langage.* Bibliothèque de Philosophie scientifique, 1936. (Ernest Flammarion, Editor.) Pp. 292.

This volume was written in a philosophical vein and in the continental tradition. Indeed it involves two continents, for the dozen

chapters represent the second part of a course of lectures given at the Collège de France (1931) and at the Université de Buenos-Aires (1932). An earlier volume on the beginnings of intelligence also appeared in the *Bibliothèque de Philosophie scientifique.*

The present volume deals with such spacious themes as concepts of quantity and of time, social relationships, and the evolution of elementary intelligence. Elementary intelligence is defined in terms of functions which lie halfway between reflex action and reasoning. Although the subject of the volume is the nature of intelligence before language, a large part of the discussion deals with the nature of language itself. The analysis of the characteristics of language leads to inferences concerning the mentation which precedes speech. In a sense Janet's approach is retro-genetic, for after a dissection of the subtleties of words and syntax, he works back to the primary stage when the functions of verbalized intelligence are prefigured on a lower, sensori-motor plane. Language is interpreted to be a form of reduced action. Words are free moving carriers and instrumental substitutes for actions. But in the primary operations which precede verbalization we have the germs of comparison, measuring, assembly, disjunction, conjunction, etc.

A favorite figure of speech for the author of this volume, which appears on many pages, is " apples-in-a-basket." This figure is used as a paradigm and explanatory device to illustrate the lower and higher operations of intelligence. The chimpanzee does not, even on a sensori-motor plane, designedly use a basket to transport apples. *Homo sapiens* does. Moreover, *homo* uses words for purposes of transportation. Words are, indeed, the most subtle, adaptable, detachable, and portable of all baskets.

Such illustrative references are characteristic of the author's treatment. He makes very few allusions to human infancy and does not attempt to base his argument on systematic factual observations. He would doubtless be interested in the recent studies emanating from Yerkes' laboratories, in which the psychological significance of the token is investigated in chimpanzee, by means of learning and conditioned response experiments. The poker chip token as a substitute or equivalent of food becomes a key to the understanding of the primitive mechanisms of non-verbal abstraction which precede language.

The philosophical essays of the eminent author of the present volume afford many glimpses into problems relating to the develop-

mental morphology of language and thought. By patient and laborious observation of infant and ape, these problems will in time yield to the methods of genetic science.

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DUNLAP, KNIGHT, *Elements of Psychology.* St. Louis: The C. V. Mosby Company, 1936. Pp. 499.

In revising his *Elements of Scientific Psychology,* Dunlap seems to fear that the younger student will not realize how novel some of the tenets, now orthodox, actually were in 1922. Actually they are likely to seem to the average graduate student of today not merely orthodox but outmoded. Behaviorism, for which Dunlap has never accepted responsibility but of which he is one of several "parents," has dulled the perception of the younger generation to the living reality of some of the problems he discusses. Since the student, despite any acceptance of the clichès of behaviorism, is certain to use such terms as "consciousness," there would seem to be need to show him that they can be used intelligibly and with reasonable consistency. This Dunlap does.

Consider, for example, the treatment of thinking. Thinking, like every psychological process, is definitely a response, but there may be involved no terminal motor pattern, either muscular or glandular. How, then, may we hold that thinking is a response? Dunlap supposes that all complete responses involve a discharge from the cerebrum to the cerebellum in addition to the motor discharge. The latter may be more and more reduced until it is finally eliminated, leaving only the efferent discharge to the cerebellum as the response of "pure" thinking. Even if "pure" thinking never occurs, this ad cerebellum discharge is the characteristic and definitive part of the total thinking pattern. The behaviorist's view of thinking as covert muscular reaction is accordingly to be rejected; a secondary accompaniment has been mistaken for the primary fact.

This is an attractive speculation designed frankly to rescue the response theory in view of the failure to find constant muscular correlatives to thinking. Dunlap discusses interestingly how it may be empirically confirmed.

There follows a clear-cut discussion of the "conscious content," the image or idea, found in thinking. Dunlap first presented this view in 1916; in the reviewer's opinion it is the only view except an out-and-out subjective idealism which makes sense. Although it is

speculative, it permits of scientific confirmation or refutation. While it is thus clearly a psychological theory, one must raise the question whether its chief significance does not lie in philosophy. Its acceptance would rule out, for one thing, a number of current epistemological absurdities. That is, of course, eminently worth while. But the theory helps very little—I think it will help some—to solve the perplexing problems of the psychodynamics of thinking.

The case with the theory of feeling is, perhaps, somewhat better. Dunlap's theory is another in the long line of those which identify feeling and sensing. In conformity with his general position, he distinguishes sharply the process from the object of feeling. The latter he calls *affect* and defines as a change in condition inside the body. Feeling is then simply a sensing of the body's own internal economy. Pure feeling is theoretically possible but in actual practice it integrates, usually as background, with other processes of sensing or thinking. In abnormal conditions the dominance of affective patterns may be frequent or persistent. We know little as yet about how to prevent such unfortunate dominance, but a clear recognition by the patient of what he is doing (*i.e.* allowing his bodily state to be excessively the primary object of awareness) is of some service.

Among the feelings, Dunlap distinguishes a special class which he calls desires. (He should say desirings to make clear the process character of what he means.) Desires, in addition to including anticipatory thinking, include awareness of a particular kind of affect, the "appet." Like the other affects, these appets are bodily conditions or changes. In the case of many of our desires, it is quite clear (to Dunlap) what the state in question probably is. But he has no suggestion as to the bodily condition for many of the most important of our desires.

To his eternal credit Dunlap does not suggest, as so many have done, a mythological derivation of these desires from those with more recognizable organic bases. Yet whether we call them desires or motivation, or urges or wants or drives or what you will, the whole of social psychology hangs in mid-air until we do find an organic basis for such things as parental tendencies, the desire for preëminence, the desire for social approval, and the like.

Now that the theory of negative practice has become familiar, there is little that is new in the treatment of learning. None the less every one of us interested in experimental work in learning can afford to read and ponder Dunlap's warning about the artificiality and arbitrariness of our procedures. For experimental purposes we

are arbitrarily isolating a large section of the animal's behavior for attention and arbitrarily exempting other sections. We must forever be on guard against ascribing the characteristics of the arbitrarily neglected segment to the arbitrarily selected segment. The conditioned response owes its vogue, Dunlap contends, to precisely this error. Of learning as conditioning he says drily, "This simplification of the complex problem of learning by abolishing it has opened up a vast field for research and publication."

It remains to make general comment on Dunlap's treatment. In the introduction he says that he leaves to the instructor the task of introducing the necessary experimental literature. That sounds fair enough and in competent hands works well. But it gives to the textbook a dogmatic tone not representative of the science and not even of the author.

In the second place, the book suffers beyond any recent book known to the reviewer from an excess of definitions and taxonomy. In one chapter, nine successive pages yield a count of 66 technical terms from biology. Many of these terms are used by Dunlap only once. Despite the fact that he lucidly explains each one, they will not be effectively learned; they merely serve to irritate and slow down the reader.

The taxonomic interest appears again and again: in the classification of the senses where it is usual but where the treatment is unusually clear, in the discussion of time, of volition, and of the forms of thinking. None of this is directly experimental, some of it not even remotely so. The temper is empirical and fact-regarding, but the author's contribution consists essentially in a dialectical rearrangement of reasonably obvious empirical data.

An enormous amount of our psychology—let us be frank—still rests upon such a basis. Lacking investigations designed to throw light on an issue, we must either accept a current (and generally merely traditional) dialectic, or we must develop the problem dialectically ourselves. In the field of sensation the old arrangement of five senses is gradually yielding to experimentally determined fact; the field of volition being practically not investigated at all, Dunlap has to fall back on dialectic to distinguish five kinds of response to which the term volition is conventionally applied. One ventures to predict, however, that the progress of investigation will show these five categories of volition to be even less useful than the five senses ever were.

To call attention to this dialectical emphasis is not necessarily to

condemn. Possibly we need it. A little proper attention to the dialectic background would certainly profit many experimenters and save them wasting time on false problems.

But merely to see what sort of a book Dunlap gives us we may contrast it with the Murphys' *Experimental Social Psychology*. Here too the authors have put themselves into the work in the way they have arranged the experimental facts, but there is a minimum of the taxonomic interest and much more interest in the situations which are dynamically connected experimentally with certain responses. Even abstracting from the copious citation of experimental data, the book could not have been composed ten years before its publication; it grows out of the experiments.

Dunlap's book, in contrast, in the main does not. Much of it could have been written by Locke or Spencer, still more by James or Stout. Essentially we have an essay—an extraordinarily capable essay—in rational psychology. One even finds one's self wondering whether this book, though truly psychological in itself, is not more a prolegomenon to philosophy than to further psychology. This is a worthy purpose. Surely the birth-pangs of psychology as a separate discipline are sufficiently remote that we may now note without shame the peculiar dependence of Mother Philosophy upon her youngest daughter. (Psychology may even some day become sufficiently "weaned" to be able to acknowledge some indebtedness to the parent, but it is still too adolescent for that today.)

Be that as it may, when you add to a strong interest in exact definition and careful classification, a dependence on dialectic and a marked emphasis upon system which, even though it remains psychology, is oriented toward epistemological problems, you have my justification for dubbing it a scholastic psychology—though without a soul.

HORACE ENGLISH.

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NOTES AND NEWS

THE SOCIAL SCIENCE RESEARCH COUNCIL has announced the award of eighty-three grants, totaling more than $100,000, for study and research in the social sciences. Among those who will receive research grants-in-aid are Floyd H. Allport, professor of social and political psychology, Syracuse University, for a study of the behaviors involved in modern culture and institutions; and Joy Paul Guilford, professor of psychology, University of Nebraska, for a study of some primary traits of personality by means of factor-analysis methods. Among those who will receive post-doctoral research training fellowships is Samuel P. Hayes, Jr., instructor in psychology, Mt. Holyoke College, for study in the United States of the psychology of politico-economic movements. Among those who will receive pre-doctoral field fellowships are Irvin L. Child, for study at the Institute of Human Relations, Yale University, of the relation of psychological conflict to cultural conflict in American-born native children of Italian immigrants; and George Kuznets, University of California, for study in the United States of the techniques of isolation of primary traits.

KARL S. LASHLEY, professor of psychology at Harvard University, has been appointed research professor of neuropsychology at Harvard and relieved of responsibility for routine instruction. He will offer a graduate seminar and direct research in neuropsychology.

H. PIÉRON, professor at the Collège de France, L. L. Thurstone, professor of psychology at the University of Chicago, and C. H. Graham, associate professor of psychology at Brown University, have been elected Fellows of the American Academy of Arts and Sciences.

THE *Psychological Record*, published by the Principia Press, Bloomington, Indiana, made its first appearance in March. Members of the editorial board are J. R. Kantor, Indiana University, editor; C. M. Louttit, Indiana University, managing editor; J. W. Carter, Jr., Indiana University, editorial assistant; and the following departmental editors: Edmund S. Conklin, Indiana University (abnormal psychology); Helen Koch, University of Chicago (child psychology); B. M. Castner, Yale University (clinical psychology); E. A. Culler,

University of Illinois (comparative psychology); J. G. Peatman, College of the City of New York (educational psychology); B. F. Skinner, University of Minnesota (experimental psychology); Norman Cameron, Johns Hopkins Hospital (medical psychology); C. F. Scofield, University of Buffalo (physiological psychology); J. P. Guilford, University of Nebraska (psychometrics); and Norman C. Maier, University of Iowa (social psychology). The *Psychological Record* is a coöperative journal, designed to afford authors immediate publication at the least possible expense. Manuscripts may be sent to the appropriate departmental editor or to the editor. Correspondence concerning editorial matters should be addressed to the editor, and business correspondence to the managing editor.

THE Psychological Institute of the University of Vienna will offer the following courses for English-speaking students in its sixth annual summer school in psychology, July 12 to August 7, 1937; *Human Personality* (Karl Bühler); *Speech and Language* (Karl Bühler); *Childhood and Adolescence* (Charlotte Bühler); *Biographical Methods* (Charlotte Bühler, Else Frenkel); *Experimental Psychology* (Egon Brunswik); *Viennese Tests for Children* (Lotte Danzinger, Liselotte Frankl); *Cases of Problem Children* (Charlotte Bühler); *Psychology of Expression* (Käthe Wolf). Further information regarding the summer school and living arrangements in Vienna may be obtained from Dr. Henry Beaumont, Department of Psychology, University of Kentucky, Lexington, Kentucky.

THE second annual meeting of the Oregon Psychological Association was held at Reed College, May 1, 1937. A symposium was held on *The Measurement and Evaluation of Personality,* and individual research reports were presented. Professor William Griffith of Reed College was chairman, and delivered the evening address, entitled *The House of Magic.* Dr. L. F. Beck of the University of Oregon was elected secretary, and Oregon Normal School was selected as the place of the 1938 meeting.